Next Meeting Dec. 19th. Subject: Braking and Traction Brakes.

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TRANSACTIONS
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OF THE

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AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, September 26, 1902.

The 168th meeting was held this date at 12 West 31st Street, and was called to order by Past-President Steinmetz, at 8:50 P. M.

PAST-PRESIDENT STEINMETZ:—Gentlemen, the meeting is called to order. The first item of business on the program of this evening, which it is my pleasure to execute, is the introduction to you of our new President, my friend Mr. Charles F. Scott. It is really unnecessary to introduce him, as Mr. Scott is very well known to you, not only by his reputation in engineering work but also by the very active part he has taken in the meetings as a member, and in the work of the Committees of the Board of Directors, to which he has belonged for a number of years. I take very great pleasure in relinquishing the Chair in favor of Mr. Scott.

PRESIDENT SCOTT:—We will proceed with our affairs in due course, and give the Secretary the floor.

SECRETARY POPE:—At the meeting of the Board of Directors this afternoon, the following associates were elected:

NAMES.	ADDRESS.	ENDORSERS.
ARMSTRONG, JAMES ROBERTS CHARLTON,	in charge of installation of switchboard at Manhattan Power Station, for General Electric Co.; residence, 140 E. 34th St., New York City.	C. T. Hutchinson. Louis Duncan. H. G. Stott.
BAYLIS, JAMES ADAMS,	Electrical Engineer, The Bell Telephone Co., Montreal, P. Q.	R. B. Owens. C. F. Sise. J. J. Carty.
BELDON, DAVID W.,	Electrical Engineer and General Superintendent, The Phœnix Light and Fuel Co., Phœnix, Arizona.	C. O. Mailloux. J. A. Lighthipe. H. A. Russell.
BENECKE, ADELBERT O., Entrance fee and dues paid	Weston Electrical Instrument Co., Waverly Park, Newark. N. J.; residence, Vailsburgh, N. J.	W. A. Anthony. R. B. Owens. Edward Weston.

BURSON, HERBERT ARTHUR, Demonstrator in Electrical Engineering, McGill University, Montreal, P. Q.	R. W. Pope. C. P. Steinmetz. R. B. Owens.
COGAN, HENRY MANNING, Electrical Engineer, The American Sugar Refining Co. of New York, Kent Ave. and So. 4th St.; residence, Shore Road and 70th St., Brooklyn, N. Y.	R. T. E. Lozier. G. F. Sever. F. Townsend.
DILLON, EDWARD PAUL, Assistant Superintendent, Colorado Department, Telluride Power Co., Telluride, Col.	V. G. Converse. G. J. Henry, Jr. L. Searing.
FINNEY, JOHN H., Assistant Manager, New York office, Stanley Electric Mfg. Co., 29 Broadway, New York City; residence, Brooklyn, N. Y.	Gano S. Dunn. C. C. Chesney. J. F. Kelly.
FREUND, HENRY PAUL, Testing Department, General Electric Co., Schenectady, N. Y.; residence, 796 Lexington Ave., New York City.	F. B. Crocker. G. F. Sever. F. Townsend.
GIBBS, HARRY PARKER, Constructing Engineer, General Electric Co., Champion Reef, Kolan Gold Field, South India.	S. B. Paine. Axel Ekstrom. C. B. Burleigh.
HARTMANN, FRANCIS M., Instructor of Experimental Physics and Electrical Measurements, Cooper Union; residence, 1428 Park Ave., New York City.	W. A. Anthony. Samuel Sheldon. D. R. Lovejoy.
HATCH, AUSTIN SMITH, Assistant General Superintendent, Public Lighting Commission, 40 East Atwater St., Detroit, Mich.	H. H. Wait. H. R. King. E. P. Warner.
HILL, JOSEPH MANSFIELD, Edison Storage Battery Co., Orange, N. J.	T. C. Martin. T. A. Edison. C. P. Steinmetz.
HODGSON, CECIL, Electrical Engineer with Stephens & Tyler, 709 Monadnock Block; residence, 3525 Grand Boulevard, Chicago.	B. A. Behrend S. L. G. Knox. R. H. Pierce.
HOLLEY, CARL HIRAM, Chief Engineer, The Mt. Whitney Power Co., Visalia, Cal.	F. G. Baum. F. A. C. Perrine. Wynn Meredith.
HUDSON, HARRY PRATT, Inspector, U. S. Engineer's Office, 5 Masonic St., New London, Conn.	W. H. Freedman. H. A. Storrs. S. F. Weston.
HUNTER, MADONE C., Electrical Engineer, Kilbourne and Jacobs Co.; residence, 1432 Wesley Ave., Columbus, Ohio.	B. F. Thomas. F. C. Caldwell. F. A. Fish.
JOYNER, ALBERT HENRY WINTER, Electrical Engineer, Edison Electric Illuminating Co. of Boston; residence, 24 Florence St., Roslindale, Mass.	C. L. Morgan. Sidney Hosmer. C. L. Edgar.

KING, R. O., 503 Markham St., Toronto, Ont.	R. B. Owens. P. G. Gossler. F. W. Roller.
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PACKARD, GRANVILLE FREDERICK, Chief Engineer, Stow Manufacturing Co., 195 Court St., Binghamton, N. Y.	C. P. Steinmetz. E. W. Rice, Jr. H. G. Reist.
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TUTTLE, HORACE BURT, Engineering Chemist, Cleveland Electrical Illuminating Co., 727 Cuyahoga Building, Cleveland, Ohio.	A. C. Middleton. Robert Lindsay. H. L. Wallau.
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YOUNG, JOHN MASON, Student in Electrical Engineering, Cornell University; residence, 302 Eddy St., Ithaca, N. Y.	H. J. Ryan. Shiro Yamazokin E. L. Nichols.

Total, 37.

And the following associates were transferred to membership.

Recommended by Board of Examiners, Aug. 8, 1902.

THOMAS E. MURRAY, Second Vice-President and General Manager, New York Edison Co., New York City.

W. S. FRANKLIN, Professor of Physics and Electrical Engineering, Lehigh University, South Bethlehem, Pa.

BERNHARD A. BEHREND, in charge Alternating Department, Bullock Electric Mfg. Co., Norwood, Ohio.

D. L. HUNTINGTON, General Manager and Chief Engineer, Washington Power Co., Spokane, Wash.

[President Scott then read an address on "Proposed Developments of the INSTITUTE." See page 1219, Aug.-Sept. issue.]

PRESIDENT SCOTT:—It has been my good fortune to have been associated in my professional work with a very able and congenial corps of associates. One man who has produced a great deal of good work has been of a rather quiet, retiring disposition. He has let his work speak for itself. But I am sure that all of those who, together with myself, have been co-workers with him will join me in saying that whatever of success and whatever of reputation Westinghouse machines may have, is due to him, both directly and indirectly, as the electrical engineers who are in the machine design work of the company have been trained up as his disciples. One the latest pieces of his work we are here this evening to have presented to us by the gentleman himself, and I take great pleasure in introducing him—Mr. B. G. Lamme.

[Mr. Lamme read his paper, entitled "Washington, Baltimore and Annapolis Single Phase Railway." See page 1231, Aug.-Sept. issue.]

Discussion.

President Scott:—The subject is open for discussion. The papers have been in the hands of some of the members for the last day or two, and there may be some who are already prepared. The Secretary has given me a list of a few members who may be called upon. We will extend the courtesy of the floor to Mr. Steinmetz, if he will please to accept it.

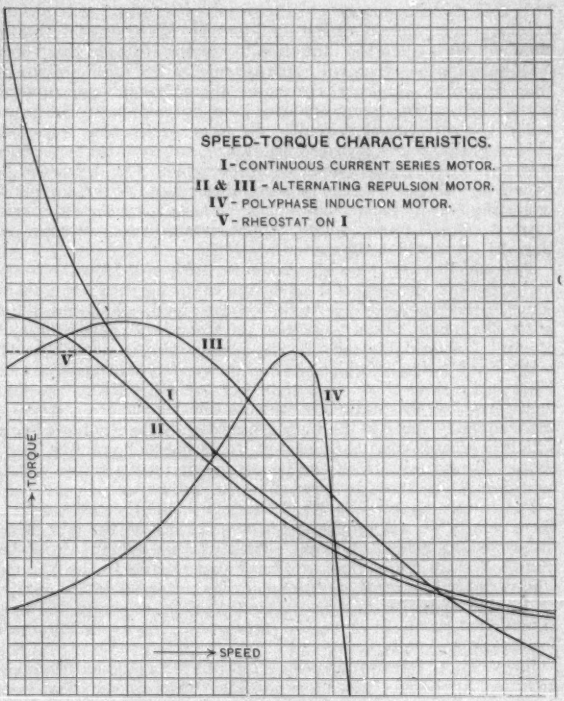
Mr. Charles P. Steinmetz:—I have read with very great interest and listened to Mr. Lamme's very interesting paper, and I believe we can congratulate ourselves that he has agreed to make public first before the Institute a description of this railway system.

It is encouraging to me that, in spite of all that has been said in past years, American engineering is still in advance. That is, while in Europe, motors which we know are not suitable to the purpose are being applied to railroad work, our engineers have attacked the problem from what I consider the only proper side, namely, the development of motors which are adapted to that work, in their speed-torque characteristics. Now, during the last few years our friends abroad have become very enthusiastic over polyphase induction motor railroading, so much so that the American press has been affected. We have been told over and over again how unprogressive we are, and that we should take cognizance of the work done by our European friends. President Scott has to a certain extent dissipated this, by showing the relative proportions of European and American electrical engineering. I may say, however, that those American engineers in whose hands were placed the destinies of large engineering manufacturing companies, went over the field in 1894–95, experimentally and theoretically, and they very completely satisfied themselves that there is no more in the polyphase induction motor for railway work than there is in the continuous current shunt motor; and since that time they have over and over again, when the enthusiasm became too high, gone over the same field, and reached the same conclusions. I believe we can congratulate ourselves then that here is published the record of some work done in the direction of developing apparatus, giving the proper characteristics for alternating current railway work. I must confess, however, that I have been somewhat disappointed in reading this paper, by seeing that after all this new motor is nothing but our old friend the continuous current series motor adapted to alternating currents by laminating the field. Now, I remember this type of motor very well because I was asssociated with Mr. Eickemeyer in 1891 and 1892, and we spent a very great deal of time in building alternating current series motors, investigating their behavior, and trying to cure them of their inherent vicious defects. The principal defect was the serious inductive sparking at the commutator, due to the short-circuited arma-

ture coil becoming a short circuited secondary with the field of the motor as primary. We did not attain satisfactory results, and I hope Mr. Lamme will succeed better than we did. I believe there is some chance for it, because it was ten years ago that we did our work, and in ten years engineering here in this country has advanced a great deal, and furthermore, we at that time worked with somewhat higher frequencies than 16 cycles which are feasible now. We found that although such a series motor inherently has a very poor power factor, we have to run it at very high speed. I am gratified to see that Mr. Lamme operates his 16-cycle motor with a speed corresponding to the frequency of rotation of something like 47 cycles, nearly three times synchronous speed; therefore, we get a good power factor.

I must confess I really expect more from Prof. Elihu Thomson's repulsion motor, in which the main current does not pass through the armature, but the armature is short-circuited upon itself through the brushes, which are shifted against the lines of polarization. Now, if you consider the problem of the alternating current motor in its derviation from the continuous current motor, you will find that by natural steps the continuous shunt motor, by adaptation to alternating currents, leads to the polyphase induction motor, and by gradual steps you are led from the continuous series motor to the alternating repulsion motor, which gives a speed-torque characteristic, very closely resembling that of the continuous series motor. You know the speed-torque characteristic of the polyphase induction motor is this, a fairly low torque in starting, gradually increasing with speed to maximum, then decreasing to zero at a definitive speed, the speed of synchronism. You may improve the starting torque by shifting the point of maximum torque, by inserting resistance in the armature and wasting the power corresponding to the difference in speed of the motor without resistance, and with resistance. Now the continuous current series motor gives a speed-torque curve something like that, starting with a definite torque also at zero speed, and decreasing gradually, first fast and then slower, with increase of speed, never reaching zero, but continuously decreasing theoretically. Probably it reaches zero when this torque reaches the friction torque. Now this is not practical in railroading, because such starting torque is beyond the torque which will slip the wheels. If we take the alternating current repulsion motor, we find the torque turning over and reaching a finite point, that is, when you come to lower speeds, the reactance of the motor begins to be noticeable, and limits the current and torque. In general you see a torque characteristic similar to that of the series motor, with rheostatic control in starting, represented by Prof. Thomson's repulsion motor without any rheostat. Now it was this alternating current single-phase repulsion motor, which I was thinking of when in some previous discussions on alternat-

ing current railroading, I mentioned the inferiority of the induction motor, and stated that alternating current railroading will become feasible only when the single-phase motor is developed, giving characteristics of the series motor, that is, starting with maximum torque and giving a torque decreasing with the increase of speed.



MR. RALPH D. MERSHON:—This paper is certainly an extremely interesting one. The development of an alternating current system of traction has long been a "consummation devoutly to be wished," and apparently the consummation is with us. I am sure that we will all watch the performance of this road with a great deal of interest, because in this case, as in most other cases, the proof of the pudding will be in the eating. Although in this

case we have apparently reached the development desired, by means of a single-phase series motor, I have not by any means lost hope for the induction motor. Not the induction motor which has been used up to date, because, as has been stated this evening and many times before, the characteristics of the induction motor as we know it to-day are those of the shunt motor, and it has the inherent defects accompanying rheostatic control; but an induction motor of somewhat different characteristics. I feel rather confident, almost confident enough to predict, that the time will come when the induction motor for variable speed work will be a motor which will, just as truly as the motor which has been described—and perhaps more efficiently—develop power at speeds below the maximum speed, in proportion to the power taken from the supply circuit. In other words, that we will be able to graduate the speed of the motor, not by means of a rheostat and thereby lose power in proportion to the difference between the full speed of the motor and the speed at which you are operating, but by means enabling us to take from the alternating current circuit, power which will be in amount closely proportioned to that which the motor develops.

I should like very much to have the author, if he will, speak a little more of the elements of design involved in accomplishing some of the results which have been attained. I should like to know the means taken to reduce the secondary sparking spoken of, and I should like also to hear from him in regard to the methods of design by which the high power factors were obtained. I am inclined to think that this was due not only to the facts mentioned by Mr. Steinmetz, that is, the high speed relative to the frequency speed of the motor, but also to the number of poles which is greater than that used in direct current practice, and I should like to ask also if the power factor results might not be considerably bettered by increasing the number of poles.

PRESIDENT SCOTT:—If Mr. Lamme will kindly make note of these points we will give him an opportunity at the end of the discussion to reply. We have with us this evening from the middle West a man whom you have just elected to the Council, who is the representative of the electrical interests in the coming St. Louis Exposition, and who has in the past year conducted some extended tests on an extensive interurban railway in Indiana—Prof. Goldsborough.

PROF. W. E. GOLDSBOROUGH:—I doubt if there is any engineer in America who has been more confident than myself, that some day we would run our railways by the alternating current. I was once so bold, a number of years ago, as to say at one of the conventions of the National Electric Light Association, when the Wagner single-phase motor was having rather a hard time, that I had a great deal of confidence in the single-phase problem surviving, and I still feel that in the end we will develop a motor which will be applicable to the railway, and which will be the single-phase alternating current motor, without the defects of

the direct current railway motor. Just what that solution will be, I am not prepared to say, because I don't know, but I think the engineers in the great factories of America, who are working on this problem, will eventually evolve something that will perform the work as I have outlined it. We are now just entering upon a new field in electric railway work. For a long time we were confronted by numerous systems for the application of direct current, and I presume during the next five or six years we will be confronted by numerous systems for applying the alternating current. In a few years from now, however, we will hear of "the standard single-phase alternating current railway equipment," and by that time there will be but one system in evidence. What that one system will be I do not believe any man here to-night knows. When that day comes I think we will have entered upon a great railway era in America. We will probably see all our trunk lines equipped with electricity. I doubt very much if that will take place prior to the time that the single-phase system is perfected for electric railway service, and I believe when that day comes that we will all be a great deal happier than we are now. The American people are essentially a traveling people, and how much pleasanter and cleaner and more delightful it will be to ride in electrically propelled trunk line trains than to be covered up with soot and cinders from our present locomotives.

I do not know that I ought to say anything that is derogatory to the steam railway problem, while under the roof of the American Society of Mechanical Engineers, yet I am certain that in this coming work, which we all foresee, the mechanical engineers will join with us in wishing it God speed, because nothing that will promote engineering from the electrical standpoint will do anything but promote engineering from the mechanical standpoint. The day is coming when the engineer will not be either electrical or mechanical; no man will be an engineer who has not a complete grasp of both sides of the profession.

It is a great pleasure for me this evening to have contributed to the discussion of the paper, and I think we all owe a vote of thanks to Mr. Lamme and Mr. Arnold and other of our engnieers who have been courageous enough to start out in this new field and *do something*.

PRESIDENT SCOTT:—Mr. Lamme has presented his paper and received the thanks of Prof. Goldsborough. Prof. Goldsborough has extended his thanks to Mr. Arnold, also, before Mr. Arnold has told us what he has developed. Mr. Arnold, by the way, is a new member of the Council, and is on the Committee on Papers for next year. He promises to do some active work in the Chicago local organization

MR. BION J. ARNOLD:—I have some photographs and drawings here in an envelope, but I do not think I will show them to-night. I came prepared to disclose additional information regarding my single-phase alternating current system provided Mr.

Lamme should show photographs or anything which would enable one to gather a comprehensive idea of his system, but he did not show us enough, and until he tells us more I think I will say nothing more about my system, as some months ago I announced the principles upon which it is built.

I do not mean by this action any disrespect to the INSTITUTE, but I would like to defer what I may say in regard to my work until a suitable time, so that I may say it in a proper way, or at any rate until I can get it so shaped that I can give full and complete information.

I did not see Mr. Lamme's paper until a short time before entering this meeting, but thinking possibly he would give more complete data or show photographs of accomplished work, I came prepared to show what I have done. I am very much interested in the development of his system, and wish to express my appreciation of his work, as I am glad to see him come out and stand for the single-phase alternating current motor for railway work. Still more pleased am I to see him, representing, as he does, a large manufacturing company in this country, come out and stand for it, for but one year ago it was not only difficult to get representatives of his company to do it, but they took a very decided stand against it and in favor of the direct current motor.

I think that my sentiments on this question are pretty well-known and can easily be ascertained by reading the records of the INSTITUTE for the last three years. I advocated the alternating current motor at the joint meeting of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS and the British Institution of Electrical Engineers at Paris in 1900; again, and against considerable opposition, at the annual convention of this INSTITUTE at Buffalo in 1901, and again last June at the Great Barrington convention, when, as is known, I announced the principles of the system that I have developed, and of which in the course of sixty or ninety days, I hope to present full details to this INSTITUTE, together with drawings and photographs of the full working system, and it is because I do not wish to spoil that paper that I hesitate about showing what I have here to night.

Now, there is one point in Mr. Lamme's paper which I am quite sure he will correct, if he thinks I am right. Rather, I would say that I do not know whether he personally intends to make the point or not, although the statement is definitely made in the preliminary articles, relating to this road, which recently appeared in the technical papers. That is, the conclusion to be gathered from his paper that this road between Washington and Baltimore is the *first single-phase railway in this country*.

In a modest and mild though emphatic way, I must resent that statement, because in December, 1899, or early in 1900, I took a contract to build complete 60 miles of railroad and use single-phase motors upon it. That road has been located,

graded, bridged, and equipped under my direction, under my own engineers and construction force, and 20 miles of it is now being operated with steam locomotives.

The trucks, cars and some of the motors are nearly ready to run, as can be shown by the photographs which I have with me and I believe that this road, when it runs, is entitled to be known as the first road equipped with single-phase motors.

Now, with this correction in Mr. Lamme's paper I have no further criticism to make, for I concur with him in his belief in the advantages of the alternating current road as described in the paper, many of which I have, from time to time, pointed out, as evidenced by the records.

I think that the essence of his paper is summed up in one paragraph, in which he says that he has succeeded in making an alternating current, single-phase motor operate in starting under load by means of a commutator, *without sparking*. If Mr. Lamme accomplished this successfully, he is entitled to great credit, and I have no doubt that he has largely accomplished it since he says so, on his experimental motor, but he has not done it yet on the scale he hopes when he gets his railway running, and if he does it efficiently on a large scale with motors of the size he speaks of operating under the varying conditions of railway work, he will deserve all the credit we can give him.

I do not know that I have another word to say on the subject, and I will, therefore, ask you to excuse me from giving any further information regarding my own system until I can present it in the proper way, and be in position to answer all questions fully and frankly.

DR. W. S. FRANKLIN:—There is one idea that has impressed me very much in listening to the paper, and that is the fact that we are going back to such old-style and well-known apparatus. It is a curious fact, I think, that early alternating current work began with such high frequencies. I think the reason of that, no doubt, is that the earlier electrical engineers worked with toy apparatus, which was necessarily driven at enormously high speed to get any output, and along with that went, of course, the necessity of high frequency. There has been a remarkable drop in frequencies in the last ten years, and the question in my mind is whether we may not see still greater drop in the future. The tendency to low frequency, of course, has been to get rid of inductance troubles, and at the same time to permit of the use of enormously large units.

PRESIDENT SCOTT:—We have with us this evening a gentleman who has been in other parts of the country, and in other countries, for a number of years. He was at one time an engineer on the Pacific coast. For the last few years he has been an engineer in charge of important transmission work in Mexico. Although he is not now connected with the INSTITUTE, he assures me that he soon will be—Mr. Norman Rowe.

MR. NORMAN ROWE:—You certainly surprise me, Mr. Scott, in

calling upon me this evening, as I had no idea of taking part in the discussion. I think those men, however, who have been with the Westinghouse Company, who know Mr. Lamme and his work, appreciate the fact that when Mr. Lamme says that he has got a motor which will develop 100 horse power, a single-phase motor that will run without sparking, and is good in all other respects, that he has there the elements of a system which will undoubtedly revolutionize street railway work, and will open a vast field in the future for trunk and interurban work. I do not care to say anything more than this. Others of you may think that possibly it is an experiment. But I know it is more than experiment; I know it is a reality.

PRESIDENT SCOTT:—It just occurs to me, gentlemen, that although this is a New York meeting of the INSTITUTE, so far there has not been a New York member who has had the floor. We want to come around to New York, and we will call on our good old friend, Mr. Mailloux.

MR. C. O. MAILLOUX:—I was wondering after Mr. Arnold spoke whether I would have an opportunity to say something, because on all occasions when the case of alternating versus direct current for railroad work has been discussed, it has happened that somehow or other, Mr. Arnold and I, good friends as we are, have differed somewhat. We still differ. There has been something said about the manner in which Amercians have been criticized for their apathy towards the alternating current system and I think that I may personally accept the responsibility for a great deal of that apathy. If you look over the TRANSACTIONS —Mr. Arnold having called attention to the TRANSACTIONS in regard to what he said, I cannot help following suit—you will find that I have been one of the skeptical engineers, who have been slow to believe in the possibilities of the alternating current system in railroad work. You will find that at a previous meeting, notwithstanding the papers by two able European engineers, both members of this INSTITUTE, which papers were calculated to show us what wonderful things the alternating system was doing in Europe, I still wished to remain unconvinced. Now, the reasons which I advanced still substantially exist. I said then that the alternating current system must undergo a great deal of development before it can hope to contend with the direct current. I admit that, this evening, we have before us the perspective of some important steps in a direction that may give us an opening towards a satisfactory solution of the problem of railway operation by means of the alternating current. In all discussions thus far but little if any attention has been paid to the single-phase, alternating current system. We have always had the polyphase system, with the induction motor especially, as the solution suggested, more, perhaps, because that is the only method which seems to have been thought of and utilized by European engineers in the many installations which they have made of alternating current rail-

road systems. It is certain that if we do away with one of the two leading conductors, that will remove a serious objection which hitherto could be urged against the alternating current system. The system proposed to-night is one which opens that perspective, and promises that feature. It still has to accomplish a great deal; it proposes to do it by presenting a motor which will have the characteristics and the working qualities of a series direct-current motor; in fact, it is nothing but a series direct current motor adapted for alternating currents. Unfortunately, the paper does not tell us very much about the manner in which the author proposes to obviate the inherent defects of such a motor. In reading the paper carefully, I failed to note that the author introduces any radically new feature, and I concluded that so far as the paper discloses ideas, one is led to suspect that he depends more upon features of design and proportion than upon original ideas, either in manipulation or control. Now we do not know, of course, what merit there may be in this; only practical experience can tell us. Mr. Lamme's friends, who seem to be here in numbers, assure us of his ability to cope with this subject. We all know and admit his reputation as a designer, therefore we may feel warranted in hoping that he has indeed found a satisfactory solution. I cannot at the same time feel quite as enthusiastic about it as either Mr. Lamme's friends, or as Mr. Arnold. I do think that it pays to be slightly conservative. I have started out to be conservative on this question, and I am going to remain so, so as to be consistent, at least. I believe that we have before us a beautiful perspective, but whether it will be a reality is something that only time will tell us, and can tell us. I am more inclined to take Prof. Goldsborough's view. I think that he takes a practical, sensible view of the entire situation when he says that some years will elapse before an alternating current railroad system can become as much standardized as the direct current railroad system, and that no one here, this evening, would be able to tell us what will be the standard alternating railroad system. There is a great deal of truth in that "conservative" statement. I also think the remarks of our Past-President, Mr. Steinmetz, are extremely suggestive, as they point out other methods which appear to be really more promising than those which are outlined in the paper. I can speak impartially because I am not interested on either side. I am very much in the attitude of one who is looking at a contest without caring much which side wins, or whether any side wins. I am perfectly willing that both sides should win. I still believe what I said the last time we had occasion to discuss this question, when I took the conservative view, namely, that there is a great deal of good work that is being done to-day, and that will be done for some time with direct current apparatus, for the simple reason that "it is better to be sure than to be sorry." It is better to

stick to the evils that we know of, than to fly to others that we know not of, and while I have a great deal more faith in the alternating current system than I had before I heard Mr. Lamme's paper, yet I am still conservative. I think it would be very interesting indeed to get the results, and I am inclined to wait for them before I become unduly enthusiastic. I hope that Mr. Lamme, or his colleagues, may be induced to give us some actual results as a sort of supplement to this paper, a year, it may be, after the road has been completed. In the meantime I would not advise those who have electric railroads to build where the conditions are first rate for direct current motors—such conditions as I have outlined in my previous discussion—namely, where you have high speed work and many stops, to wait for new systems, but to use direct current motors. It may be that these can also be improved. It may be that stimulated by the forthcoming competition of the alternating current motor, our good friends who have been directing their time to the direct current motor may also find some way of making them do a little better than they are doing now.

PRESIDENT SCOTT:—Are there any others who desire to take part in the discussion?

MR. JOSEPH SACHS:—Assuming that both sides will win, which is probably a reasonable assumption, the matter of detail is a question that will arouse a little attention, if not from the strictly scientific end, at least from the man who runs the road. I have been very much interested recently in the matter of satisfactory suspension apparatus for high potential railway wires, and am very anxious to hear from Mr. Lamme what method he has adopted for suspending the 1,000-volt wire for the railway system that he speaks of. Whether he has stuck to the old methods of insulation, or whether he has adopted something else.

MR. WILLIAM C. GOTSHALL:—When Mr. Arnold began to speak, I became very much interested. I was really afraid that he was going to give away some salient point of some new system. When Mr. Mailloux spoke, I became even more interested, and, if anything, more alarmed, for I feared Mr. Mailloux was going to give away something relating to a system which is being developed in connection with an electrically operated high-speed railroad, which I have something to do with. Mr. Mailloux is eminently qualified to speak of the alternating current system referred to in Mr. Lamme's paper, or, in fact, upon any lternating current system. Mr. Mailloux and I spent about a year trying to find out whether the alternating current could be used on a certain railroad. The results of our conclusions were that we would have to use direct current.

On arriving here this evening, I discovered a gentleman from the Middle West with whom it was my good fortune to be associated about nine or ten years ago in St. Louis. We were both interested in developing the business of a large alternating current station, and in connection therewith spent considerable time in

an attempt to develop a single-phase alternating current motor. We found that the motor was entirely applicable for very small powers, that is, for fan purposes. Incidentally, we learned a good deal about the single-phase alternating current motor, all of which knowledge is possessed by the gentleman to whom I refer. This gentleman, I believe, knows as much about alternating currents as anybody in the country, and I would like to have the Chair call upon Mr. Herbert A. Wagner to add to this discussion by stating some of his experiences with the single-phase alternating current motor.

PRESIDENT SCOTT:—The suggestion is most appropriate—Mr. Wagner.

MR. HERBERT A. WAGNER:—After such an introduction I hardly know what to say. I have had some experience with alternating current motors, and it gives me the greatest pleasure to see a dream, which I have had for years, about to be realized in the form of a successful alternating current, single,-phase railway motor. I have always been an advocate of single-phase alternating current motors, and spent a great many years in experimenting to develop such motors, with some measure of success, for stationary work. My experiences during years of experiment in that line could hardly be told in a few minutes. Some of them were very discouraging. Most of them, in fact. I happen to know that Mr. Lamme has not experimented with such a motor for a few months only. I think I remember many years ago in Pittsburg, in the early nineties, probably, of seeing a street railway motor mounted on trucks, and some brake tests being made. I was told that the motor or motors—there were two, I believe—were single-phase alternating motors with commutators, the motors being very similar to the direct-current railway motors then being made by the Westinghouse Company, adapted to the use of alternating currents. I did not know what success was achieved with those motors at that time, but I did know that they were not put into commercial use for some reason or other.

With Mr. Steinmetz I think that there is also a future, a very promising future, for the type of motor known as the Thomson repulsion motor. I have experimented to a great extent with that class of motor myself. I know that the sparking, especially at low frequencies, can be overcome, and I have no doubt that that motor can be developed, in time, for street railway work. I am also pleased to see the introduction of single-phase, rotary converters. I think some years ago in a paper read before the National Electric Light Association, I made the statement that there would be on the market before many years a single-phase rotary converter, and that the design of such a machine ought not to be much more difficult than that of the polyphase, although the output would of course be somewhat less for a given amount of material. I should like to ask Mr. Lamme about some of the details of the construction of his

motor, particularly with reference to the air gap; whether it is not necessary in order to obtain his high power factor to use a very small air gap; also, I should like to know if the field of the motor is constructed with the ordinary polar projections of the direct current motor, or whether it is a ring provided with slots into which the windings are placed, and if so, whether the field winding is a distributed winding or a concentrated single coil winding.

MR. ELIAS E. RIES:—At previous meetings of this INSTITUTE a number of papers were read and more or less discussion indulged in with reference to the operation of railways, particularly trunk line railways, by means of alternating currents, and on all of those occasions, as the TRANSACTIONS will show, I have uniformly been one of the foremost advocates of the alternating current motor for that class of work. One peculiarity of the installation now proposed is the fact that it happens to take place on the line between Baltimore and Washington—Baltimore being the city in which I formerly lived—and I remember that some years ago, I think it was in 1893 or 1894, there was a movement on foot to build what was called at that time a " Boulevard Electric Railway " between those two cities, over what was then a record distance, the proposed road being about forty miles long.

I had previously given considerable study to the question of applying alternating currents to operate long distance electric railways, having taken out the first patents ever granted on this subject, which as a result of my experiments in this direction, I applied for as early as 1887.

When this boulevard road was proposed, it was the intention of its projectors to operate it by means of the direct current, using a three wire transmission system with a difference of potential of about 1,000 volts between the two outside wires. I felt satisfied however, that this road, if built, could not be operated with any commercial success upon that plan, and that a system of transmission and distribution involving the use of a high tension alternating current transmission line with either static or rotary converter sub-stations was the only feasible one.

I finally proposed a method for operating that line by means of a single phase, alternating current system. I figured out the details, and in many respects they were substantially similar to what we have listened to to-night. That is to say, I proposed using a 5,000 volt alternating current transmission circuit supplying energy at high tension to about eight converter stations, using, preferably, static transformers to transform the primary voltage to a low secondary or working pressure of about 500 volts, and also using a series motor with laminated fields. I likewise designed and recommended as an alternative type, a constantly running synchronous motor, provided with a variable pressure friction clutch mechanism for starting and controlling the speed of the car. There were a good many difficulties, however, in the way, that being at a very early stage of the art, and I

therefore did not succeed in making much of an impression upon the experts employed by the road, which it subsequently turned out, was never completed. In view of my own experience I am very much pleased to learn to-night from Mr. Lamme's paper that he has gone so thoroughly into the details of the single-phase alternating current railway system that I then proposed, particularly in the matter of regulation and adjustment of the car motor conditions, which from the experiments he has made, lead him to believe that a single-phase motor, on the plan of the old series motor, is apt to be productive of good results.

In September, 1896, I believe, a paper on the same subject was read before this INSTITUTE by Dr. Louis Duncan, with whom I was in consultation at the time of this proposed Baltimore equipment, and in the discussion following the reading of that paper I also took an active stand in regard to the advisability of using alternating current single-phase motors as soon as a commercially practicable motor—better than those with which I have experimented—was forthcoming or was put upon the market by the large electrical manufacturers. I might say that I have always taken an active interest in the matter, as I was one of the pioneers in the art of operating railways by means of alternating high-tension transmission currents, that are converted at intervals along the line of way by means of suitable apparatus into low-tension working currents which are then fed to the car motors. For that reason, among others, I am very glad to see that the matter is receiving the attention that it is at this time. There have been a good many skeptics among our members in the past, as has been stated here to-night, but I was never one of them; I always felt confident that the time would come shortly when my early ambitions and faith in this method of operation would be realized and when we would see the single-phase alternating current system, with its manifest advantages over the direct-current system, taking first rank in matters of railway operation.

PRESIDENT SCOTT:—If there are no others who care to take part in the discussion, we will give the floor to Mr. Lamme.

MR. LAMME:—I have only a few comments to make. Some doubt has been expressed in regard to the sucecss of A. C. single-phase motors of 100 h.p. size, which is that to be used on the Washington, Baltimore and Annapolis Railway. It is stated in the paper that motors of this size have been tested. The engineers of the Cleveland Construction Company selected the 100 h.p. size because they had actually seen them in operation in the factory.

Several speakers have referred to this as a proposed system, but we wish to call attention to the fact that it is not simply a scheme, but a system which is so far advanced that the contract has been taken and the apparatus is being manufactured. It is not something which we hope to do, but something which we are doing.

In the discussion this evening, reference has been made to the repulsion motor, which is the type in which the field is connected across the line and the armature brushes are short-circuited on themselves and set at an angle to the ordinary neutral point. This type is mentioned on page 1236 in the August and September number of the TRANSACTIONS. The opinion has been expressed by several speakers that the repulsion type of motor is better suited than the straight series motor for the service proposed. We have investigated both types of motors and consider that the repulsion type is not so suitable as the straight series. There are some very serious disadvantages with the repulsion type of motor in this class of service. The construction is more complicated as the reversal of the direction of rotation is not so easily accomplished as in the plain series motor. Analysis of the motor shows that there are other important objections which will count heavily against it. According to our calculations, the repulsion motor will be larger and heavier for same output. It appears, therefore, that the series motor is the more suitable one for railway work.

Some questions have been asked in regard to the high power factor obtained with the motors to be used on this railway system, and it has been suggested that the large number of poles and the low frequency had something to do with the good power factor. In answer it may be stated that the frequency, of course, will have considerable to do with the power factor of the motor but there are many other points entering into the construction that have an effect on the power factor. There are so many of these points that no one of them should be credited with any great gain. The entire proportions of the motor have to do with the results obtained as regards power factor. With a 4-pole motor a good power factor can be obtained—with a 12-pole motor perhaps slightly better can be gotten. It is probable that the 4-pole motor would be so nearly like the 12-pole in many features that it would be difficult to note the difference, but in some particular points either one would show some advantage over the other. The number of poles chosen was that which appeared to give the best average results.

The opinion has been expressed this evening that we are practically going back to an old, well-known type of motor. Looking at the motor as a whole, it may appear to some that there is nothing new, but nevertheless here is a plant which will be put in operation with a type of motor which some consider to be old, but which accomplishes a result that has not been attained before. That fact indicates that there must be some features in this apparatus which have not been utilized before.

Some reference has been made to the single-phase rotaries. One reason why single-phase rotaries have not been used heretofore has been due to the fact that there has been comparatively few occasions where their use was necessary. Rotaries have been called for principally in connection with polyphase plants.

Polyphase rotaries, of course, possess some advantages over single-phase; but the difficulties of construction of the single-phase rotaries are not great, and where there has been occasion for such machines, and supply conditions have been suitable, it has been the practice of the Westinghouse Company to bid on them. Polyphase rotaries have been frequently operated single-phase for temporary purposes. There are cases where they have been operated single-phase for several weeks at a time with satisfactory results.

Mr. Arnold has called attention to the fact that this is not the first example of the single-phase railway. So far as I know, this is the first example in which the scheme of operation is a purely electrical one. If there are other roads ahead of this one, of course this road cannot have the credit of being the first.

MR. MERSHON:—Mr. President, Mr. Lamme has omitted answering some of the most interesting questions, the question of sparkless commutation, the question of air gap and the question of type of field winding, are not only of great interest from the designer's standpoint, but of great interest from the practical standpoint. It is to be regretted that Mr. Lamme cannot see his way clear to give us fuller details.

MR. STEINMETZ:—Before you conclude, I should move a vote of thanks to Mr. Mailloux for his contribution to the library, which establishes a precedent in supplying us not only with books, but also with an endowment to take care of the further issues of those periodicals.

The motion was carried.

PRESIDENT SCOTT:—Mr. Mailloux has the thanks of this INSTITUTE.

MR. MAILLOUX:—I rose primarily to speak on the paper. Having been honored by the kind vote of the gentlemen present, I would first make a few remarks on this subject. I am more than repaid by your generous appreciation. I thank you. I know that there are many who can do at least as well, and many very much better. I hope they will all do it, and all do it immediately. We have a society that has been growing very fast. It has had a brilliant past, but it has and should have a much more brilliant future. Let us all help to make it brilliant. As the President stated, we ought to have—and figures will not lie—25,000 members. I hope we will get them. One of the ways of getting them is to get a fine library, and a fine building. This meeting, with its overflowing attendance, has shown the necessity for a fine and large building. We cannot accommodate the members in a building of this size. We ought to have one twice as large. I hope you will all go home and think about it, gentlemen, and that your thoughts will bear fruit.

Now about the paper. Generally, I want to state that the reason why some consulting engineers like myself are skeptical and conservative is, possibly, because those who are interested in designing and introducing alternating current systems are mysterious and uncommunicative about them. I must state that while I am skeptical, I have never been lacking in openness

to conviction. I think you will find in all the discussion wherein I have participated, that I have always manifested a disposition to accept progress and welcome it. I have stated in previous discussions that there are many cases for which I was desirous of finding an alternating current system. I would hardly welcome it, and I am very glad to see progress evidenced this evening. At the same time I must say that I would be better satisfied if we could get more complete and satisfactory technical information as to the features whereby the mysterious and wonderful results enumerated are to be obtained. I have previously stated that there did not seem to be anything radically new in the paper which could account for these results, and that they must be accounted for, if at all, by certain peculiarities of design and proportion, and it seems to me that we are missing the heart of this discussion, as Mr. Mershon has intimated, when we fail to elicit information as to those very important points.

PRESIDENT SCOTT:—Mr. Lamme may have the floor again.

MR. LAMME:—The information which has been asked for in regard to the constructional features of the machine will probably be given at a later date. At present there are good reasons why it is not desired to give some of that information. When the proper time comes the whole of it may be given.

MR. WAGNER:—I asked one of my questions, with reference to the air-gap, as having a bearing on the mechanical construction and operation of such a motor. If the air-gap has to be very small, it is a mechanical question as to how often the bearings will have to be renewed to prevent the armature from brushing the field. Such questions, engineering questions like that, I thought might be answered. There was one other question that I wished to ask. Of course if Mr. Lamme prefers not to answer, very well. I want to know if the motor can be reversed without resetting the brushes; that is, can a position be found for the brushes which is equally good for either direction of operation. I do not wish to pry into any details of electrical design which may not be fully covered or protected at this time.

MR. LAMME:—The question has been asked in regard to the air gap on the motors for the Washington, Baltimore and Annapolis Railway. The clearance over the armature in these motors is practically the same as that between the pole piece and the bands in ordinary D. C. railway motors. The question has also been asked whether it is necessary to shift the brushes of these motors when the direction of rotation is to be reversed. In answer, it may be stated that the brushes are on the true neutral point as in D. C. railway motors and the brushes are not shifted when the direction of rotation is changed.

MR. P. K. STERN:—I believe that I understood Mr. Lamme to say the brushes were set in a position 45° from the neutral point.

PRESIDENT SCOTT:—The reference is to another type of motor, I think. If there is nothing further, gentlemen, the motion to adjourn is in order. [ADJOURNED.]

PITTSBURG LOCAL MEETING.

MINUTES OF MEETING HELD FOR THE ORGANIZATION OF A LOCAL BRANCH OF THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, AT PITTSBURG, PA.

In accordance with the suggestion made by President C. F. Scott in his presidential address and on invitation from the Electric Club, Mr. C. W. Rice, Chairman of the Institute Committee on Local Organization, called a meeting for the organization of a local section of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS in Pittsburg, to be held in the Lecture Hall of The Electric Club, Monday evening, Oct. 13th.

The meeting was called to order by Mr. C. W. Rice.

Mr. Rice was elected Chairman of the evening and Mr. C. E. Skinner, Temporary Secretary.

The following resolution was adopted:

Resolved, That a committee of five (of which the present Chairman shall be one), be appointed to call a second meeting, to provide a programme therefor, and to propose a plan for a permanent organization.

The Chair appointed Mr. P. M. Lincoln, Prof. S. M. Kintner, Mr. F. B. Erwin, Mr. J. S. Peck and Mr. Calvin W. Rice.

Chairman Rice gave a short talk on the desirability of those engaged in electrical pursuits becoming members of the INSTITUTE and read sections of the Constitution, giving the requirements of membership and the initiation fees and dues.

Mr. Scott, President of the INSTITUTE, was then called upon to read his address entitled, Proposed Developments of the INSTITUTE, which was delivered on his inauguration as President at the general meeting of the INSTITUTE in New York, September 26, 1902. Mr. Scott elaborated those points which were of special local interest.

Chairman Rice then called upon Mr. P. M. Lincoln to read Mr. Lamme's paper entitled " Washington, Baltimore & Annapolis Single-Phase Railway." Mr. Lincoln abstracted Mr. Lamme's paper, giving the more important parts. In conclusion, Mr. Lincoln called attention to the fact that Mr. C. P. Steinmetz, at the New York meeting, agreed in practically all engineering points presented in Mr. Lamme's paper. Mr. Lincoln stated that the extra losses in the rails are not really serious, as these can be reduced by increasing the voltage of the circuit, thereby reducing current. He stated that the extra losses in the rails are caused by the so-called " skin effect " when magnetic materials are used as conductors. Measurements made on the Manhattan Railway showed, under certain conditions, an increase in the line loss of about 400 per cent. when magnetic material was used on the circuit over that of non-magnetic material.

Chairman Rice then called for questions on the paper of the evening. These questions were answered by Mr. Lincoln.

Q. Why is the power factor variable?

A. For the reason that the inductive element of the input to the motors at any given current is a constant, whereas the power element is proportional to the speed. It is evident, therefore, that the ratio of these two quantities—which is the power factor—will increase with increasing voltage and therefore increasing power output.

Q. What is the power factor of the motor when starting?

A. From 30 to 35.

Q. Why are motors connected in series parallel instead of all in series?

A. Chiefly to reduce the voltage across the motors and therefore to reduce the strains on the insulation of the armatures.

Q. How is the induction regulator operated?

A. By air pressure.

Q. Why is the primary of the regulator connected in series with the motors?

A. To reduce current in the regulator and auto-transformer and therefore make them smaller.

Q. Why are the fields of the motors connected in separate circuits instead of directly in series with the armatures?

A. In order to enable the motors to be reversed by a single switch.

Q. Why are balancing coils used across the motor armatures?

A. In order to make use of static friction. With this arrangement it is not possible to slip the wheels to which one motor is connected without slipping all, and maximum tractive effort is therefore available.

At this point Chairman Rice called on Mr. N. W. Storer for a few remarks.

Mr. Storer stated that during the last few months, as the exponent of the D. C. railway system, he had been the recipient of much advice and many pitying glances, implying that he would soon be out of a job and had received many offers of new positions as the D. C. railway motor was sure to become a thing of the past. He said that in spite of this, the D. C. railway motor was still doing business and as far as he could see would continue to do so for some time to come. He also said that he welcomed the advent of the new A. C. railway motor as it solved one of the difficult problems in connection with railway work, viz., the long distance railway lines. With D. C. railway motors it is not practical to use more than 600 volts across the motor, or 700 volts on the line, on account of the liability to flashing at the commutator. He said he was particularly gratified to find that when the A. C. motor was to be adopted, the designers were compelled to use the old D. C. motor. There would be some disadvantages in the new system which did not occur in the old, one of these being the increased weight necessary, consisting of transformers and other

apparatus on the car. Mr. Storer closed by saying that he was very glad to extend the right hand of fellowship to the new system.

Mr. Scott spoke of the fact that in the beginning there was a wide difference between the appearance of alternating and direct current machines, but that these differences have gradually become less, until at present there is a very close similarity, so much similarity that it is sometimes very difficult to distinguish one from the other during construction in the factory; and that it is notable that after so many attempts at alternating current railway work, the present motor is remarkably similar to the old direc-current series wound motors. He said that the paper by Mr. Lamme was more than a statement of a new motor; it was a broad statement of a complete new system, indicating how many things are required in the development of such a system, and how many things must be worked together in order to produce an operating system. If, for instance, the drop in the rails should happen to be several times greater than it is with the alternating current at the frequency selected, the present scheme might not be feasible. Mr. Scott referred to his introduction of Mr. Lamme at the New York meeting, where he said that Mr. Lamme had had to do either directly or indirectly with the development and success of practically all the machinery manufactured by the Westinghouse Company.

Chairman Rice asked that the members be perfectly free in giving to Mr. Lincoln, Chairman of the Committee of Permanent Organization, any ideas they might have in regard to such an organization.

A count of the INSTITUTE members present showed a total of 19, with approximately 120 Club members and guests.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, October 24, 1902.

The meeting was called to order by President Scott at 8.30 P. M.

THE SECRETARY:—At the meeting of the Board of Directors this afternoon, the following associates were elected:

Name.	Address.	Endorsed by
BAINTON, JOHN RICHARD,	Consulting Engineer, Edge and Edge, 250 Pitt St., Sydney, New South Wales.	J. S. Fitzmaurice. Arthur J. Hanson. Wm. Maver, Jr.
BEAM, VICTOR SHAEFFER,	Electrical Engineer, Board of Patent Control; residence, 1 W. 129th St., New York City.	C. A. T erry. C. F. Scott. F. N. Waterman,
BEATTIE, MARK BREWER,	General Electric Co., 706 Phœnix Building, Minneapolis, Minn.; residence, Ann Arbor, Mich.	James Lyman. Henry S. Carhart. G.W.Patterson,Jr.
COLLINS, CURTIS C.,	Electrical Engineer, S. A. Luz Electrica, Box 244, San Juan, P. R.	J. R. Lovejoy. C. D. Haskins. F. C. Caldwell.
COPP, FRANK TOULMEN, JR.,	Testing Department, General Electric Co.; residence, 12 North Ferry St., Schenectady, N, Y,	C. P. Steinmetz. E. B. Raymond. A. L. Rohrer.
DAVIS, WILLIAM GRIFFITH,	Power and Mining Eng. Department, General Electric Co.; residence, 14 North Ferry St., Schenectady, N.Y.	C. P. Steinmetz. H. G. Reist. A. L. Rohrer.
DUDLEY, EUGENE ELMER,	Chief Electrician, American Steel and Wire Co., Georgetown; residence, 880 Millbury St., Worcester, Mass.	W. E. Haskins. Wm. Maver, Jr. Townsend Wolcott
FAIRCHILD, WALTER LOWE,	Sales Engineer, Stanley Electric Mfg. Co., 29 Broadway; residence, 102 West 81st St., New York City.	C. C. Chesney. W. S. Franklin. F. A. C. Perrine.
GODBE, MURRAY CHARLES,	Department Engineer, Utah Light and Power Co., Salt Lake City, Utah.	L. S. Boggs. T. B. Whitted. P. H. Thomas.

GOODY, CORAL PAYNE, Assistant Engineer, The Telluride, Power Co. Logan, Utah.	Wm. Maver, Jr. Townsend Wolcott F. W. Roller.
GRAY, GUTHRIE, Assistant Mechanical Engineer, Department of Works, World's Fair, St. Louis, Mo.	G. F. Sever. P. M. Lincoln. C. R. Huntley.
HALL, FRANK WELLS, Office Engineer, The Sprague Electric Co., Room 209, Fisher Building, Chicago, Ill.	G. H. Hill. Wm. Maver, Jr. F. A. Scheffler.
HAMILTON, JAMES HENRY, Electrical Signaling Engineer, The Cape Government Railways, Cape Town, South Africa.	John Denham. J. E. Lloyd. A. W. K. Pierce.
HASELDEN, HARRY ARIEL, Electrical Engineer, The Whitin Machine Works, Whitinsville, Mass.	C. B. Burleigh. S. B. Paine. D. R. Bullen.
HENDERSON, JOHN SAMUEL, Superintendent and Electrician, City Electric Light Plant, Brewton, Ala.	Wm. Maver, Jr. Townsend Wolcott F. W. Roller.
HERBERT, EDWARD, Western Electric Co., 259 South Clinton St.; residence, 111 Loomis St., Chicago, Ill.	C. W. Pen Dell. H. R. King. C. E. DeCrow.
HOXIE, HALL FARRINGTON, Electrical Engineer, Board of Patent Control; residence, 96½ Hicks St., Brooklyn, N. Y.	W. J. Jenks. E. W. Hammer. C. A. Terry.
JACOBSON, JULIUS R., Special Employee General Electric Co., Schenectady; residence, 310 Almond St., Syracuse, N. Y.	C. P. Steinmetz. A. L. Rohrer. E. B. Raymond.
JOHNSTON, RICHARD HARRY, Assistant, R. D. Lillibridge, 170 Broadway, New York; residence, 972 Park Place, Brooklyn, N. Y.	R. D. Lillibridge. T. C. Martin. G. F. Sever.
JOLLY, JOHN MACCALLUM, Electrical Engineer, Noyes Bros., 109 Pitt St., Sydney, N. S. W.	W. G. T. Goodman. J. S. Fitzmaurice. A. J. Hanson.
JONES, BENJAMIN NEEDHAM, Electrician, Marine Eng. and Machine Co., Harrison; residence, 46 Amherst St., East Orange, N. J.	F. A. Scheffler. Wm. Maver, Jr. Gano S. Dunn.
JONES, ROBERT CLAY, Turnbull & Jones, Electrical Engineers and Contractors, Box 362, Dunedin, New Zeal.	W.G.T.Goodman. J. S. Fitzmaurice. Wm. Maver, Jr.
JOSLIN, ARBA VANDERBURG, Transformer Inspector, Ind. Electric Light and Power Co.; residence, 213 Elm Ave., San Francisco, Cal.	F. G. Baum. F. A. C. Perrine. R. S. Masson.
KAPPELLA, ADOLPH SOMERS, Head of Induction Motor Tests, General Electric Co.; residence, 1010 State St., Schenectady, N, Y,	C. P. Steinmetz. A. L. Rohrer. E. B. Raymond.
LIVERS, JOHN LEO, Electrical Contractor, Martinsburg, W. Va.	T. C. Martin. C. R. Van Trump. R. W. Pope.
LODYGUINE, ALEXANDER, Electrical Engineer, National Battery Co., 359 Normal Ave.; residence, 86 Grant St., Buffalo, N.Y	Carl Hering. Joseph Wetzler. Wm. Maver, Jr.

MacKeen, Rupert Thomas,	Assistant Superintendent of Construction, The Canadian General Electric Co., Toronto. Ont.	E. M. Archibald. R. B. Owens. Wm. Maver, Jr.
Medbury, S. C., Jr.,	Electrical Engineer of Stations Va., Passenger and Power Co., 116 N. 3d St., Richmond Va.	E. M. Archibald. E. W. Trafford. E. J. Berg.
Nesbit. William,	Salesman Westinghouse E. & M. Co., 902 University Block, Syracuse, N. Y.	C. F. Scott. J. S. Peck. P. H. Thomas.
Replogle, James Gillespie Blaine,	Superintendent, The South Fork Electric Light Co., Box 164, South Fork, Pa.	Wm. Maver, Jr. Townsend Wolcott F. W. Roller.
Scott, Robert Julian,	Professor of Engineering and Electricity, New Zealand University, Canterbury College, Christchurch, New Zealand.	W.G.T.Goodman. J. S. Fitzmaurice. A. J. Hanson.
Stone, Charles Leroy,	Power and Mining Department, General Electric Co.; residence, 31 North Ferry St., Schenectady, N. Y.	F. O. Blackwell. W. H. Sawyer. A. P. Jenks.
Waters, William Lawrence,	Chief Engineer, Christensen Eng. Co.; residence, 656 Astor St., Milwaukee, Wis.	B. A. Behrend. F. G. Bolles. L. E. Bogen.
Williamson, Robert Baird,	Principal of School of Electrical Engineering, The International Correspondence Schools; residence, 406 Taylor Ave., Scranton, Pa.	H. S. Webb. H. J. Ryan. W. S. Franklin.
Woolford, William Allen,	Testing Department, General Electric Co., Schenectady, N. Y.	C. P. Steinmetz. A. L. Rohrer. E. B. Raymond.

Total, 35.

The following named Associate was transferred to full Membership. Recommended by Board of Examiners, Friday, Sept. 12, 1902.

George Walter Greenwood, Jalapa Railroad and Power Co., Jalapa, Mexico.

The President:—The Chairman of the Reception Committee, Mr. T. Commerford Martin, has some remarks to make.

Mr. Martin:—At the request of Mr. Scott, I have a great deal of pleasure in reporting to the members of the Institute, the work which has been done by members who were selected to coöperate with members of the other three national engineering societies, with regard to the founding of the John Fritz medal for achievement in the industrial arts and sciences. This movement had its inception last spring. This is Mr. John Fritz's eightieth year, and it was proposed to signalize that interesting event by the foundation of this medal, Mr. Fritz being, as many of you gentlemen know, the oldest living iron master and engineer in this country connected with the great underlying arts of the production of iron and steel. A general committee was formed. Its first meeting was held in these rooms, and that meeting was

attended by your Chairman of Committee on Papers, Mr. Calvin W. Rice, by the Secreatry, Mr. Pope, by Mr. William Maver, Jr., and by myself. Mr. Rice, as you know, has been called away from New York to assume very important duties. Our esteemed Secretary, Mr. Pope, had other pressing engagements, and I seemed to be the only member left of the Committee who had any leisure. The various committees of the General Committee were formed, and I have much pleasure and pride in reporting that we have raised a fund, to which the electrical engineers have contributed largely and liberally, of $6,000. The John Fritz medal to-day is an assured fact. We have paid out about $1,500 to Mr. Victor D. Brenner, an American sculptor, for the execution of the medal, and we have enough left in hand to provide a fund of about $150 a year, which will allow of the presentation of a gold medal every year of the value of about $100. It is proposed that the medal shall be awarded by a select committee of four members from each of the four national engineering bodies, four from the electricals, four from the mining, four from the mechanicals, and four from the civils. Should any society fail to coöperate in any given year, it is still proposed that the award of the medal shall take place if anybody is found deserving; and the remaining three or remaining two societies will constitute the necessary committee and executive body to award the medal. As far as I am aware at the present time, there is not the slightest doubt as to the coöperation of the four national engineering bodies in this very interesting and important movement. I believe it is the first time we have ever coöperated in anything of the kind, and that it should have achieved such an instantaneous success is an augury of the best character with regard to our mutual working together and coöperation in the future. I think it is necessary for the success of each of the four national bodies, that they should stand together rather than stand apart. So far as our own INSTITUTE is concerned, I understand that the following gentlemen will represent this INSTITUTE on the committee or council which is to award the medal. I presume it will be awarded within one year from the present time. The members of that committee are Mr. Charles F. Scott, Mr. Charles P. Steinmetz, Mr. Carl Hering and Dr. A. E. Kennelly. I am sure you will agree with me that no more appropriate selection could possibly be made.

Having achieved the first part of our purpose in the foundation of the medal and in having it executed by the best artistic skill that we could command in America, the next thing to do was to celebrate the event in fitting fashion. The John Fritz Medal banquet will be held in this city at the Waldorf-Astoria next Friday night, Hallowe'en, and the indications are that it will be one of the memorable banquets in this city. There are already subscriptions to the dinner for about 450, and as we are still a week away from the event, I think it is safe to say that we shall have at least 500 or 600 ladies and gentlemen present upon that

occasion The representation of the INSTITUTE there is excellent and could hardly be better, but we are naturally desirous that the electricals should show up as strongly as they can on that occasion, and if any of you gentlemen wish to be present, I would like personally, in view of the honor which has been conferred upon me of making me a member of the Dinner Committee, to ask you to be present upon that occasion and to help hold up my hands. I want to see the electricals as strongly in this movement as possible, and I live in hopes of seeing the John Fritz medal conferred upon some of the members of the INSTITUTE, upon some of the gentlemen whom I have the honor to see before me at the present time.

The subscriptions to the John Fritz Banquet will be received by Mr. J. C. Kafer, the President of the Engineers' Club, at the Club, not later than Wednesday next, and from present indications, we shall have to close the list a great deal earlier. Personally, I feel the utmost pride and pleasure in the success of this first joint movement amongst the National engineering bodies of this country, and I live in hopes, also, of seeing medals like the John Fritz medal founded in our own SOCIETY.

THE PRESIDENT:—Mr. Martin is not only a member of the Dinner Committee, but he has the position to which he is entitled by proved ability. He is chairman of that committee. The Board of Directors of the INSTITUTE decided this afternoon that the INSTITUTE should be represented on the Board of Award of the John Fritz medal, by the three most recent past presidents, and by the present president. It is the intention each year to appoint the incoming president as a member of the Board of Award, there being one change each year.

The subject of the meeting this evening is " Photometry and Illumination."

INTRODUCTION.

THE PRESIDENT:—It is difficult for us whose streets and houses are illuminated almost as well by night as by day to realize that it was only a trifle over a century ago when inventors began to make improvements upon the primitive lamp which had done service for many thousands of years. The fuel for lamps was principally solid or fixed oils until the advent of mineral oils within the past fifty years. The general change from candle-sticks to lamp chimneys was in the days of our fathers and mothers. Gas lighting became common in London in 1816. The electric lighting on a commercial scale has come about with the easy recollection of all of us. It was the demand for illumination and the development of electric lighting which gave the electrical industry its great impetus and it is still one of the most important branches of electrical work. If time permitted, it would be interesting to study the influence of artificial illumination as a factor in civilization, to note its effect upon domestic affairs, social customs and industrial activity.

Light as a commercial product must be measured. Although it is one of the most common commodities and has been for years sold daily to millions of customers, yet there is scarcely any commodity in which the standards and methods of measurement are so inadequate. Gas is sold by volume and not by the quantity of illumination produced. Electricity is sold by the quantity of current or of energy, which is probably quite satisfactory to the central station. When, however, the measurement of the light itself is considered, the methods and the standards are inadequate and often illogical; for example, the candle power of an arc lamp has been so unscientifically stated that as a makeshift it is often designated by the watts consumed irrespective of the actual quantity of light or of the difference in the quality of light from various lamps.

Although the measurement of the invisible, intangible, subtle flow of electricity is effected with the greatest degree of refinement and precision by instruments which are the embodiment of simplicity in design, construction and operation and the electrical units of measurement are among the most definite physical standards, yet the measurement of light and of illumination is one for which the standards are not simple and convenient and for which the methods of measurement are difficult and inconvenient, and unsatisfactory and inadequate. Peculiar difficulties are inherent in the problem, as it involves physiological as well as physical elements. The only purpose of illumination is the production of a certain effect upon the nerves of the retina, and popularly speaking, it is the amount of this effect which is termed the intensity of the illumination. The color and character of the light, as well as its quantity, are elements of the first importance both objectively when considered in a physical and in a scientific sense and also subjectively in their relation to vision. These effects are by no means the same with different individuals nor are they constant with the same individual.

In discussing the operation of incandescent lamps on low frequency alternating current, the late Prof. Rowland once remarked that the proper test would be to have an old lady use the light for reading and see whether it hurt her eyes. At first this suggestion seemed rather out of place, but I rather think that the leading physicist of America was not far wrong when he proposed the physiological rather than the physical test to determine the suitability of a source of light.

In a scientific sense the quantity of the energy which is luminous and which is capable of affecting the retina is the important element when determining the efficiency of a source of light. This luminous energy is quite small compared to the heat energy which is wasted energy so far as the eye is concerned. In a physical measurement of the efficiency of illumination the total quantity of light emitted is the only quantity to be considered. In determining this it is necessary to obtain the total radiation and it is for this purpose that the mean spherical or the average

candle power in all directions is measured. In general, it is not total illumination which is of practical consequence; it is effectiveness in illumination which is wanted, and not so many total candle power or so many watts consumed. Effective illumination involves quantity of light, color of light, distribution of light and the canons of adequate and acceptable illumination are physiological as well as physical. Two sources of illumination which emit the same aggregate quantity of light cannot be placed upon a commercial parity if one of them throws a greater proportion of its light up when the illumination should be downward or vice versa; nor if one gives yellow light which is less effective than the white light of the other; nor if one emits its light from a single intense source, whereas the other may send its light forth from numerous points or an extended area.

It is well to study carefully and make intelligent use of the physical methods of measurement. Such methods are certainly useful in comparing different lights of the same kind or order and they greatly assist one who is experimenting to increase the efficiency of a given type of lamp. But in our study of the standards to be used in the measurement of light and the methods and apparatus which are suitable it is fitting that we should have in mind the general problem of illumination. We must recognize that although electric lighting is of prime importance in contributing to the general welfare and in its commercial aspects, nevertheless, all of our present methods of producing electric illumination require an exorbitant expenditure of energy for the production of a given quantity of light on account of the large amount of waste heat; also that the present methods of supplying light usually give a distribution which is uneven, ineffective and unsatisfactory; and also that the present methods of measurement, although susceptible of a fair degree of physical precision do not adequately measure what is of first importance, namely, the effectiveness of illumination.

A paper read at the 169th meeting of the American Institute of Electrical Engineers, New York, October 24, 1902.

AN INTEGRATING PHOTOMETER FOR GLOW LAMPS AND SOURCES OF LIKE INTENSITY.

BY CHARLES P. MATTHEWS.

In a previous paper[1], I have described an equipment designed for the photometric study of arc lamps. The most valuable feature of this equipment lies in its ability to yield a value of the mean spherical luminous intensity from a single photometer setting. It is the purpose of this paper to describe an apparatus possessing this same valuable feature, and several others worthy of note, when used for the photometry of the incandescent lamp and other sources of the same order of brightness.

Designed especially for incandescent lamp measurements, the apparatus has several functions and might have been styled a "universal glow lamp photometer." It is capable of use for all photometric measurements on the glow lamp that do not lie in the province of spectro-photometry. To particularize, it may be used as follows:

(1.) As a simple photometer for any unidirectional measurements, such as occur in standardizations, ratings and candle-power distributions.

(2.) As an integrating instrument for the direct determination of mean horizontal, mean spherical, mean hemispherical or mean zonular candle-power.

(3.) As an integrating instrument for the direct determination of the *spherical reduction factor*; that is, the ratio—mean spherical: mean horizontal candle-power.

1. TRANSACTIONS, xviii, 1901

The Importance of the Mean Spherical Value.

The fact has long been recognized[2] that the only strictly fair basis for the comparison of incandescent lamps, is that of the total flux of light emitted, a quantity proportional to the mean spherical candle-power. It is possible by altering the configuration of the filament to concentrate luminous flux in particular solid angles at the expense of flux in other angles. Hence, two lamps of equal rated candle-power may yield total light flux in quite different amounts. On the other hand, if two lamps have initially the same mean spherical candle-power, their relative value is determined simply by their power consumption and sustained candle-power. The vexed question of what is the most useful light may well be left to the purchaser, who can select the type of filament best adapted to his own needs. The question is comparable to that which asks: What is the best diet for man?

Existing Methods for the Determination of the Mean Spherical Value.

Of the methods in use in the determination of the mean spherical candle-power, the most accurate is that in which measurements are made at equal angular intervals through 180° in a plane passing through the axis of symmetry of the filament, the lamp being rotated meanwhile about this axis. From the readings so obtained, the mean spherical value may be found either by formula or the graphical construction known as the Rousseau diagram.[3]

The method employed in the Franklin Institute tests of 1884 involves the mean of 38 candle-power values taken in such directions at to give a nearly uniform space distribution. The mean of these values is the result sought.

Both of the foregoing methods involve a large number of readings—so large, in fact, that their application to the practical rating or extended study of lamps is out of the question.

A third method consists in the use of the spherical reduction factor appropriate to the type of filament under consideration. Unfortunately, this factor is not a constant for any given type

2. In 1897 a committee of the Institute reported as follows: (Transactions, page 90.) "Although incandescent lamps are at present rated by their horizontal candle-power, yet, since the only true criterion of the total quantity of light emitted by a lamp is its mean spherical candle-power, we recommend that the rating of lamps should be based upon their mean spherical candle-power so far as is commercially practicable."

3. Transactions, September, 1901

of lamp. With some types, the method yields a good result; with others the variations are such as to render the results only roughly approximate. A disadvantage is the large and increasing number of types on the market and the necessity for determining and keeping account of the corresponding constants.

Liebenthal found as the result of an extended series of measurements that the mean of the intensities taken at 51.8° north polar distance and 51.8° south polar distance on a spinning lamp is a value approximatihg the mean spherical intensity regardless of the type of lamp. The errors resulting from the application of this method range from —.1% to + 3.9%. This is probably the most accurate of the simpler approximative methods.

THEORY AND DESCRIPTION OF THE APPARATUS.

From what has been said, it would appear that there is need of a photometer capable of giving the mean spherical candle-power of an incandescent lamp with the ease and celerity obtainable in the ordinary photometric measurement. With this need in mind, I have designed and had constructed the apparatus decribed below.

The theoretical basis of the design is the approximate equation for the mean spherical intensity

$$I_{ms} = \frac{\pi}{2n} \sum_0^{\pi} I_\theta \sin \theta \qquad (1)$$

wherein I_θ is the intensity of a ray making an angle θ with a vertical passing through the light center and n the number of terms in the summation. In order that equation (1) may apply to a glow lamp, it is necessary to spin the lamp precisely as is commonly done in determining mean horizontal candle-power.

To see how equation (1) may be made the basis of an integrating photometer, let us consider a source of lights (Fig. 1), and a photometer screen P whose plane extended contains the effective light center of S. We will hereafter denote the center of the photometer screen by p and the effective light center by q. For convenience we will further assume that p and q lie in the same horizontal plane to which the plane of P is normal, and we will call the line $p\,q$ the axis of the system. Now consider two mirrors whose planes are vertical and make an angle of 90° with each other. Let the centers of these mirrors, designated by a and b, come into the horizontal plane in such positions that the lines $q\,a$ and $p\,b$ are equal and respectively normal to the axis

of the system. Let a' and b' be the centers of a second pair of mirrors occupying a position such as would be found by swinging a and b, without mutual displacement, upward about $q\ p$ as an axis until $q\ a$ makes the acute angle θ with the vertical. Having thus located a' and b' angularly, we may now assume that some radial movement of this pair of mirrors is possible. The eye placed at p will see virtual images of the source in horizontal and θ aspects respectively. The images s' and s'' may be regarded as producing jointly an illumination on the photometer screen of

$$i_0 + i_\theta = \frac{K_0\, I_0}{d^2_0} + \frac{C\, K_\theta\, I_\theta}{d^2_\theta} \tag{2}$$

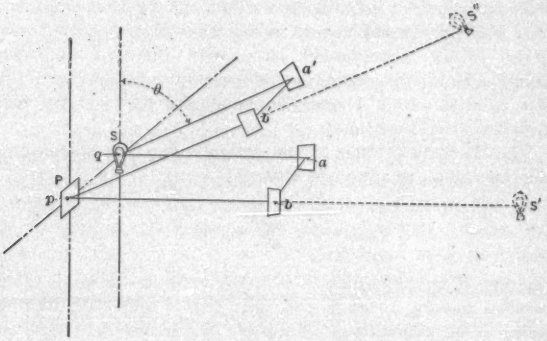


FIG. 1.

where K_0 and K_θ are the reflection co-efficients of the pairs of mirrors, d_0, d_θ, the distances from source to screen by way of the mirrors and C a factor varying with the incidence of the light upon the photometer screen P. Now if n pairs of mirrors be placed similarly to a, b and a', b', but spaced at equal angular intervals of $\Delta\ \theta$ such that $n\ \Delta\ \theta = \pi$, we shall have as the resulting illumination

$$\sum_0^\pi (i) = \sum_{0'}^{\pi} \frac{C\, K_\theta\, I_\theta}{d^2_\theta} \tag{3}$$

If by radial adjustment of the mirror pairs we make

$$\frac{C\, K_\theta}{d^2_\theta} = \frac{K_0\, \sin\, \theta}{d^2_0} \tag{4}$$

then

$$\sum_0^{\pi}(i) = \frac{K_0}{d_0^2}\sum_0^{\pi} I \sin\theta \tag{5}$$

That is to say, the total illumination of the screen is proportional to the mean spherical intensity of the source. (See equation 1.) To evaluate this intensity, it is necessary merely to balance this illumination against that due to a source of known intensity at a known distance.

Fig. 2 shows the disposition of twelve pairs of mirrors, m_1, m_2, etc., in order to produce the desired results. If the illumination of a given surface varied exactly as the cosine of the incidence of

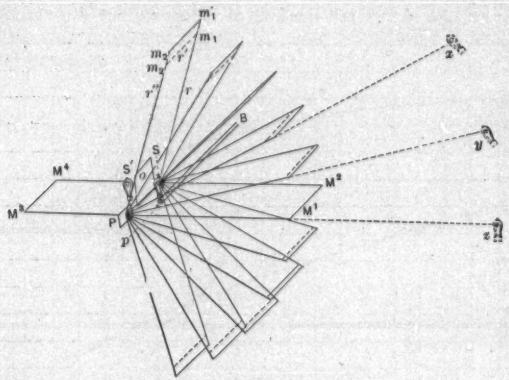


FIG. 2.

the light upon that surface, and if all mirrors used were of equal reflecting power, then there would be no need of radial adjustment of the mirror pairs, for in such case $S\,C = \sin\theta$ and $K_\theta = K_0$. But the so-called cosine law is only approximately true and mirrors vary in reflecting power, hence it is necessary to compensate for discrepancies in C and K_θ by slight changes in d_θ. The extent to which this correction is of importance depends, of course, upon the nature of the photometer screen. The plaster of Paris surface of the Lummer-Brodhun screen obeys the cosine law with exactness up to an incidence of 50°, but beyond this point a divergence of increasing magnitude occurs (Fig. 3). Hence, for the Lummer-Brodhun screen the only adjustment of

mirrors necessary is that to overcome variations in their reflection co-efficients except for angles greater than 50°. With mirrors cut from one sheet of glass, the correction for variation in reflecting power is often negligible. With the Bunsen screen, correction must be made for all angles of incidence. For example, the second curve in Fig. 3 is the result of measurements made upon ordinary draughting paper from which the Bunsen screen is often made. Here the departure from the cosine relation is noticeable from the very beginning and becomes as high as 15% at 75° incidence. The third curve in the same figure shows the results obtained with ordinary glazed writing paper. The cosine relation is not even roughly approximate is this case.

Fig. 2 also shows the method of balancing the illumination

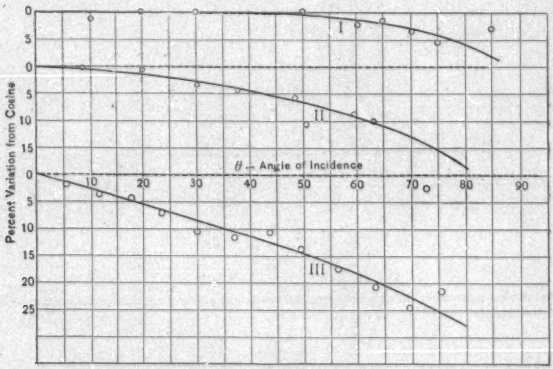


FIG. 3.

Per cent. variation from cosine relation for different screens.
I.—Lummer-Brodhun screen.
II.—Unglazed paper.
III.—Glazed paper.

produced by the series of images due to the circular system of mirrors. Mirrors M_1, M_2, M_3, M_4 are cut from one piece of glass just as are the mirrors in the ordinary Bunsen photometer. It is not essential that the other mirrors of the system should have the same coefficient, since, as already explained, the initial adjustment corrects for failure of the cosine relaton and inequalities in the mirror coefficients at the same time. With the standard at s′ separated from s, the source to be tested, by an opaque screen, a balance in the illumination is obtained by moving M_3 and M_1. The method of doing this will be better under-

stood by reference to Fig. 4, which shows in elevation and plan the essential elements of the apparatus. To the right of these figures is seen the mirror system, each pair of mirrors being capable of a certain amount of radial movement for purposes of the initial adjustment of the instrument. s is the lamp to be tested, mounted upon a rotator, s′ is a standardized glow lamp. The mirrors M_3 and M_3, rigidly connected, may be moved along the bar by means of a rack and pinion conveniently under control of the observer at the photometer P. The photometer is fixed and hence the operation of making a setting is more convenient than that which obtains with the ordinary sliding form. As the design is based upon the approximate equation (1) and not upon the integral form, some error arises from this cause. With eleven pairs of mirrors the error is negligible for all practical purposes.

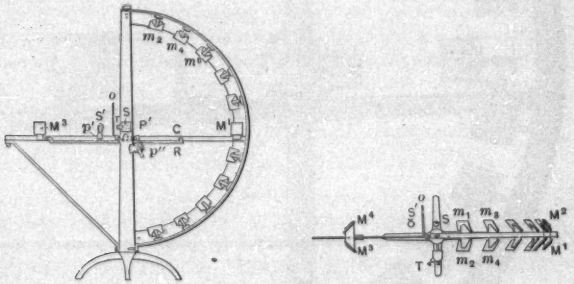


FIG. 4.

Fig 5 shows a photograph of the finished apparatus with all screens removed in order that the details of construction may be better seen. This particular instrument is fitted with a Lummer-Brodhun photometer screen. Each mirror bracket is provided with two pins. These pins extend through the frame of the ring radially. By means of this construction, each mirror pair may have independent radial adjustment.

We will now consider in detail the different operations to which the instrument readily lends itself.

OPERATION 1.—MEASUREMENT OF MEAN HORIZONTAL CANDLE POWER.

In this operation the apparatus is used as a simple photometer. Hence, all mirrors except the four horizontal ones are covered

by black screens suitably provided for the purpose. The right pair of mirrors (Fig. 4) is connected at c to the sliding rod carrying the rack. The lamp s to be tested is mounted in the rotator

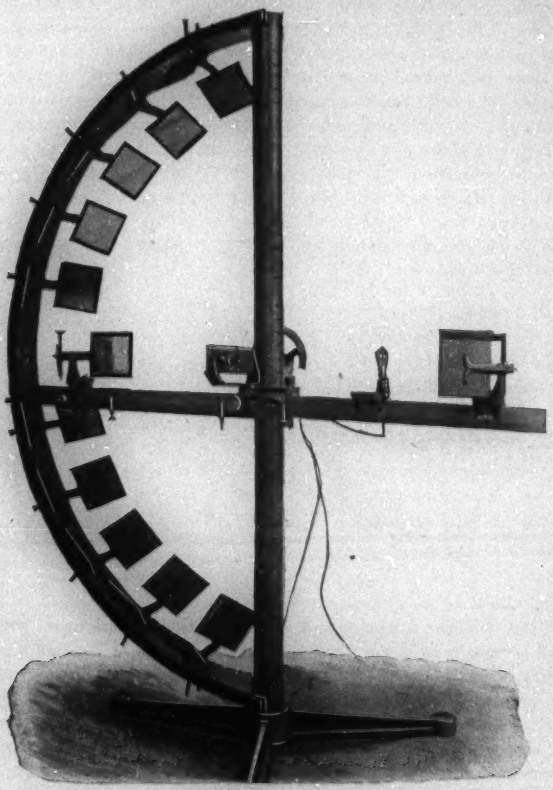


FIG. 5.

and driven at a speed of say 180 r. p. m., and a standardized incandescent lamp, of intensity I, is placed in a suitable holder, at s′. The rod R may be moved by the hand for a rough adjustment, and the pinion P″ used only for the final setting. Since a

displacement d of the mirror pairs means a change in the light paths of $2d$, the moving rod is graduated in divisions one-half the unit (centimetres) in which the light paths are conveniently measured. A reading R means that the distance from S′ to the screen by way of the mirrors M_3, M_4, is R centimetres. If, now the total photometric distance between the sources is 300 $c\,m$, we have for the intensity I of the light under test

$$I_s = \left[\frac{300 - R}{R}\right]^2 I_{s'} \tag{6}$$

$$= T_1 I_{s'} \tag{7}$$

where T_1 is the value of the expression in brackets and stands for "tabulated value corresponding to the reading R." When the apparatus is used with a standard of always the same intensity, it is a simple matter to make the instrument direct reading.

Obviously, the mean intensity in any north or south polar zone may be found by clamping the arm of the rotator at the proper angle and spinning the lamp, readings being taken as for the mean horizontal measurement. The mean horizontal candle-power of a flame source must be found by taking the horizontal distribution step-wise since it is impracticable to rotate such a source. Equation (7) is applicable in such measurements.

OPERATION 2.—DISTRIBUTION OF CANDLE-POWER IN VERTICAL PLANES.

The distribution of intensity of an incandescent lamp in any vertical plane is obtained with the apparatus arranged as described under Operation 1. The arm of the rotator is merely tilted about a horizontal axis so as to bring any desired aspect of the lamp toward the photometer screen. From equation (7) the different intensities are easily found.

The vertical distribution of candle-power from a flame source cannot be obtained by tilting the arm of the support. The following method is available in such cases: Mount the burner vertically at S, Disconnect the mirrors M_1, M_2 from the movable rod and push them to their place at the extreme right (Fig. 4). Now with a standard of the same order of brightness as the source to be tested, make a setting. The horizontal intensity is given by

$$I_0 = \left[\frac{200}{R}\right]^2 I_{s'} \tag{8}$$

$$= T_2 I_{s'} \tag{9}$$

where T_2 is the tabulated value of the expression in brackets corresponding to the reading R.

To obtain the intensity of the source in a direction of θ_0 to the vertical, the horizontal and θ mirrors should both be used. This prevents the limit of the bar being reached by mirrors M_3, M_4. The intensity is given by

$$I_\theta = \frac{T_2 I_{s'} - I_0}{\sin \theta} \tag{10}$$

where I_0 is the intensity found in the horizontal measurement.

OPERATION 3. STANDARDIZATION OF GLOW LAMPS.

For this operation the lamp s is removed and a horizontal circular plate mounted in place of the lamp. This plate is ruled with concentric circles which facilitate the centering on the amyl-acetate lamp or other primary standard. The lamp to be standardized is mounted at s′. With all mirrors screened except the horizontal ones, and with M_1 M_2 attached to the bar, settings are made as usual. The value of s′ is given by

$$I_{s'} = \frac{1}{T_1} \tag{11}$$

if the standard is unity.

OPERATION 4. MEASUREMENT OF MEAN SPHERICAL INTENSITY.

(*a*) *Glow Lamp.*—The lamp to be tested is mounted in the rotator and driven at a speed of about 180 r. p. m. Mirrors M_1 M_2 are detached from the movable rod and pushed to the extreme right, in which position they may be considered a part of the system of eleven mirror pairs. With a standardized lamp at s′, a setting in made in the manner already described. If R is the reading, we have

$$I_{ms} = T_3 I_{s'} \tag{12}$$

where T_3 is a tabulated value corresponding to the setting R and $I_{s'}$, the intensity of the standard as heretofore. Thus the operation has all the simplicity of any photometric measurement.

The intensity of the standard used should be approximately that of the lamp to be tested. For example, if a 16 c.p. lamp is to be tested, a standard of not less than 16 c.p. is best. With such a standard, the range of possible measurement depends upon the limit of travel of the mirrors M_3 M_4. A 16 c.p. lamp will serve as a standard for the measurement of mean spherical intensities ranging from 2 to about 25 c.p., when the limit of

travel is about one metre. It is best to substitute a 32 c.p. standard for intensities much greater than 16 c.p.

(*b*) *Flames.*—To obtain the mean spherical intensity of a flame or of any source that cannot be rotated, it is necessary to repeat Operation 4 at equal angular intervals on the horizontal circle. The mean of the results may then be taken.

OPERATION 5. DIRECT MEASUREMENT OF THE SPHERICAL REDUCTION FACTOR.

If the standard s′ and the opaque screen o be removed, it is clear from the figure that the left side of the photometer screen will be illumined by the horizontal rays of the lamp s. In fact, if s be rotated, we will have on the right side of the photometer screen an illumination proportional to the mean spherical intensity of s and on the left side of the screen an illumination proportional to the mean horizontal intensity of the same source. Under these conditions the photometer setting yields the spherical reduction factor. In other words, the mean spherical intensity is measured against the mean horizontal intensity as a standard. The reduction factor is given by

$$f = T_2 \qquad (13)$$

As the removal of the standard s′ lengthens the distance from source to screen, it is necessary to add a constant to the reading. This is provided for by a second reading point marked R F. All readings for Operation 5 must be taken at this reference mark.

OPERATION 6. TO CHECK THE HORIZONTAL MIRROR CONSTANTS.

As before stated, it is essential that the four mirrors attached to the movable bar should have the same constant. To ascertain if this condition exists, remove the opaque screen and the lamp s′ as in Operation 5. Connect the horizontal mirrors to the moving rod and, screening all other mirrors, take reversed photometer readings on a rotating lamp s. If the mirror coefficients are equal the mean reading will be 150, indicating equal light paths on each side of the photometer screen. Here again the R F reading mark must be used.

OPERATION 7. TO CHECK THE ADJUSTMENT OF THE CIRCULAR MIRROR SYSTEM.

In case of any doubt as to the accuracy of the initial adjustment of the photometer, or in case of the substitution of a new screen, it may be necessary to readjust the mirrors of the half ring. This operation is best performed as follows: Mount at s a 32 nominal c.p. lamp, and at s′ an 8 c.p. lamp, the latter

being in circuit with a rheostat capable of continuous variation. With mirrors M_1 and M_2 free from the moving rod, and with all other mirrors on the half-ring screened set R equal to 100 c.m. Now, while maintaining the 32 c.p. lamp at constant voltage, vary the voltage impressed on the 8 c.p. lamp until the photometer shows equal illuminaton. Note voltage on 8 c.p. lamp. Repeat measurements with reversed photometer. The lamp should finally be maintained at the mean voltage so found. Under these conditions the two lamps have a candle-power ratio of 4:1. Next tilt the lamp holder to an angle of 15°. Uncover the corresponding mirrors ($\theta = 75°$ or $\theta = 105°$) and cover the horizontal ones. Under these conditions we have the same aspect of the lamp toward the photometer, but with the light incident at 15°. Set the bar at

$$R_{15} = \frac{R_0}{\sqrt{\cos 15°}} \tag{14}$$

Adjust the 15° mirrors radially until an equality of illumination is obtained, then secure them by means of the set screw. This operation may be repeated until all the mirrors have been adjusted.

In conclusion, I would acknowledge my indebtedness to Messrs. D. M. Lynch and E. D. Fristoe for their painstaking labor in construction a preliminary form of this apparatus. I am also indebted to Mr. C. R. Dooley, Assistant in Electrical Engineering at Purdue University for assistance.

A paper presented at the 109th Meeting of the American Institute of Electrical Engineers, at New York, October 24th, 1902.

DISTRIBUTED LIGHTING.

BY DOUGLASS BURNETT, B.S.

The papers of Messrs. Bell[1], Doane[2] and Ryan[3] presented this year to the INSTITUTE, direct attention to the possibility of reducing the general considerations which they propound to a more scientific and exact basis than has yet appeared.

Commercially speaking, the instances are few in which the raw material is the finished product; and the higher the civilization, the greater the number of developing steps and the greater the efficiency of each.

The necessity of manipulating sources of light to practical ends has been realized for many centuries and the early developments are illustrated in the Tissot paintings, which show a number of ancient lamps grouped on super-posed trays.

Photometric studies have been concerned with sources of light, their production, comparison and standardization, their intensity quality and characteristics; and with the instrument for measuring their candle power, both total and intrinsic, as well as with simple formulas for theoretical cases of light distribution.

Most experiments have been made in rooms or enclosed spaces from which practical conditions have been excluded as much as possible, the walls being plain and made non-reflecting by dead-black surfacing, and scientific refinements have so far been applied that even the atmosphere in which the standard flames have been burned has been replaced by one of pure oxygen.

I wish to direct thought to the necessity of determining—not the conditions existing at the prime source of light, but those existing at the illuminated surfaces with which we are constantly concerned in daily life.

No light may ever be used until it impinges upon something, for, unless interrupted, light will continue along a straight line indefinitely. Distributed sources of light and diffusing surfaces are always present in actual lighting. The percentage of time which one spends and of work which one does in direct sunlight in city life is very small; artificial lights should be and are usually so screened or arranged as to take advantage of the diffusion and minimize the glare in one way or another. These results are secured by applying more or less ornamental shades to naked flames, by frosting incandescent lamps or surrounding them with diffusing screens and directing shades and by enclosing arc lamps and gas lamps of high intensity in translucent globes.

These processes are carried to an extreme in cases of "concealed lighting," where all the lamps are hidden from view behind solid cornices or translucent materials. In New York City there are several excellent instances of halls or rooms lighted entirely in this manner, using incandescent lamps.

The indications are that such methods may be applied to a greater extent than heretofore in the event of developments in vacuum tube or gas tube lighting. This type of lamp should lend itself particularly to diffused lighting effects, owing to the greater area of the source of light and its correspondingly decreased intrinsic brilliancy; the result would be a decrease in the spotting effect, which has been difficult to eliminate with incandescent lamps, owing to the brightness of the wall surface in the immediate neighborhood of the lamps. The optical efficiency is also high, being as much as 32 per cent. for Geissler tubes, compared with 5 per cent. to 10 per cent. for incandescent, and 15 to 25 per cent. for arc lamps.

It would be interesting to compare the intrinsic brilliancy of the light of such tubes with that of other lights; the figures are for an arc crater 6,400 candle power per square centimeter[5], for a Nernst glower about 100[6], for an incandescent filament about 40 and for an Argand gas flame .3 candle power per square centimeter. In connection with these figures it may be said that the arc lamp is much too dazzling for exposed use ordinarily and even with the incandescent lamp the after-image is quite prominent. It is seen that the development of lights of high intrinsic brilliancy renders increasingly important the study of the distribution and diffusion of their light.

The inverse square law for determining the amount of illumination on any surface is based on the conception that the light

from an isolated source radiating into space decreases in proportion to the areas of concentric spheres. In practice the amount of illumination is greater than would be determined by this law, but is less than that obtained from a non-divergent beam, such as that from a searchlight projector, in which case were it not for the intervention of dust, fog or other obscuring particles, the illumination upon a standard reflecting surface would be the same at any point in the beam within an infinite distance.

The increase of the illumination upon a lighted surface due to diffusion from reflecting surfaces near the source of light, reaches a maximum represented by the expression $\frac{1^{\text{s}}}{1-f}$ where f represents the reflecting coefficient and is the fraction showing the percentage of reflected to the incident light. If f equals 95 per per cent., the multiplication would be twenty times. Some actual tests of this effect are needed, though reflection coefficients have been measured.[9] In consequence of this multiplying power, diffusing surfaces are therefore a positive aid in distributing illumination.

The illumination apparent upon a surface is, according to Fechner's law, proportional to the logarithm of the actual lighting stimulus reflected from the surface.

The distribution of illumination along a surface with reflecting power of f per cent., from a lamp of intensity I candle power, located at a normal distance of h feet from the surface, throwing rays of light at angles θ with the surface, the rays reaching points x feet distant from the foot of the normal, is determined by the formula

$$e = \frac{f I \sin \theta}{h^2 + x^2}$$

"e" is to be measured in luxes or candle feet. In this connection question may be raised as to the desirability of calling the lighting effect of one lux on a unit surface, otherwise called a lumen, one " ray " or one " beam " of light, just as in connection with the magnetic field we use the term " line of force."

It is therefore possible in some measure to calculate the distribution of lighting along an illuminated surface, though the problem becomes further complicated when a second reflecting surface is introduced and it may be at present considered imprac-

ticable to determine with any fair degree of accuracy the actual illumination in a room.

It seems, however, quite possible actually to measure this lighting effect.

In several instances such curves of illumination have been plotted [10], but are usually based upon the assumption that there is no reflcetion or diffusion. In others, the mistake has been made of plotting the candle power of the light incident upon the surface, with the result of exaggerating the apparent lighting effect.

A fundamental law in this connection is that illuminations from independent sources may be arithmetically added to determine resultant illumination.

An error, which I think has always been made, is in connection with the plotting of the space intensity curves of incandescent, arc, Nernst and other lamps. The impression they convey would much more nearly accord with what actually occurs if the curves showed--not the intensity of light in the various directions, but the lines along which the lighting is equal at any point. Such curves may be called "isophotals," and while they are of the same general shape as intensity curves, their dimensions are as the square root of the radii.

A theoretical set of such curves has been published by Weber.[11]

The tendency of enclosing an arc, for instance, either in the interior gas globe or more particularly in the outer diffusing globe, is to make such an isophotal curve approach more nearly a circle, which effect is further enhanced by the diffusion from a neighboring ceiling; thus tending towards the desired result of a flat curve, indicating uniform illumination.[12]

Arc lamps for the lighting of country roads should be such as throw their lighting downward; whereas in the lighting of city streets, lined with buildings, such lamps should be used as will throw an amount of light on the walls for diffusion purposes as will not be objectionable to the occupants.

A number of photometers for determining the intensity of sources of light have been devised, while few instruments, notably those of Weber[13] and of Houston and Kennelly[14] for the measurement of illumination on a surface have been described; and in each of these the lighting was not measured directly, but was adjusted by means of screens or by means of the weakening of visual power, and then compared with a standard amount of illumination. The nearest approach to attempt such a

measurement has been made, I believe, by Hutchinson[15], in connection with the lighting of the Congressional Library at Washington, but in this case a simple photometer with a Bunsen grease-spot disc was used, one side being exposed to the illumination of the room, the other to an adjustable standard of light—in this case a 16 candle-power lamp, but in that case the candle-power was measured and the illumination estimated from the known distance of the group of lights, no allowance being made for diffusion; and all measurements were made in a single position.

The Bunsen, the Lummer-Brodhun, and other standard photometers operate with varying and indeterminate amounts of lighting on the surfaces to be compared; and any one of them is in essence a luminometer rather than a photometer, since the candle power is deduced by the application of the mathematical formula instead of measured or compared directly.

The author has devised an instrument arranged so as to expose a standard white surface to the ordinary lighting of a room, the amount of which is not varied. The position of a small incandescent lamp in reference to a similar and adjoining surface is, however, adjusted in order to secure within commercial limits an equal illumination, the position of this standard lamp being indicated on a graduated scale in terms not of distance but directly in terms of the number of luxes. Other observers[16] have stated that the use of small incandescent lamps is quite permissible for such purposes for several hundred consecutive observations. The only precaution necessary in the use of such an instrument would be to shield the eyes from light other than that reflected from the two standard surfaces, and to shield each respectively from the other source of light.

It may be of interest at some future date to plot the space candle-power curves from a standard open arc without enclosing globe, the space candle-power curves of an enclosed lamp of the same watt consumption with the diffusing globes used in practice, the isophotals from the latter lamp, the theoretical illumination from the open arc along a horizontal surface without diffusion and the actual illumination curves from the lamp with the enclosing and diffusing globes in a room under normal operating conditions.

In addition to the case cited there have been but few instances where the amount of illumination on a surface has been mea-

sured or calculated, notable among which may be mentioned a test of the lighting of the streets of Paris.[17]

Our methods of expressing illumination have been in terms of the number of standard lamps such as those of 16 candle power 50 watts per hundred or thousand square feet of surface. Bell[18] has expressed an opinion that 5 candle-power per square inch is a maximum for interior lighting; Cohn[19] has given 10 meter candles as the hygienic minimum and 50 meter candles as the value of daylight; and Wybauw[20] has given 15 to 25 meter candles as necessary to fluent reading, or one meter candle for street lighting. Possibly practical requirements may hereafter be more clearly specified by the assistance of some such instrument as has been suggested in this paper.

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A paper presented at the 169th Meeting of the American Institute Electrical Engineers, New York, October 24, 1902.

SOME METHODS OF PHOTOMETRY AS APPLIED TO INCANDESCENT LAMPS.

BY J. T. MARSHALL.

For fifteen years or more the Edison General Electric Company and its predecessor, the Edison Lamp Company, have tested lamps for voltage on a so-called sliding scale photometer. This type of photometer is an adaptation of the ordinary form of a Bunsen grease spot photometer, by which the voltage of the lamp at rated candle-power is determined from the candle-power it gives at a given voltage.

If two lamps of the same candle-power and voltage be placed one at each end of the photometer and the same voltage be applied to them, they will give the same candle-power and the photometer screen will balance at the middle of the photometer. If the two lamps are of different voltages, they will give different candle-powers and the screen will balance at some other point than the middle of the photometer. The relative candle-power of the two lamps, as indicated by the point at which the screen balances, will determine their relative voltage. The voltage of one lamp being known, the voltage of the other lamp may be read directly from a scale on the photometer made to suit the lamp.

The sliding scale photometer does not require a volt-meter—an advantage which, before the introduction of reliable dead-beat volt meters, was more apparent than at present. This type of photometer has a further advantage, in that its readings are not materially affected by changes of several volts in the line—for the reason that these changes similarly affect both lamps, and do not alter the balancing point of the screen.

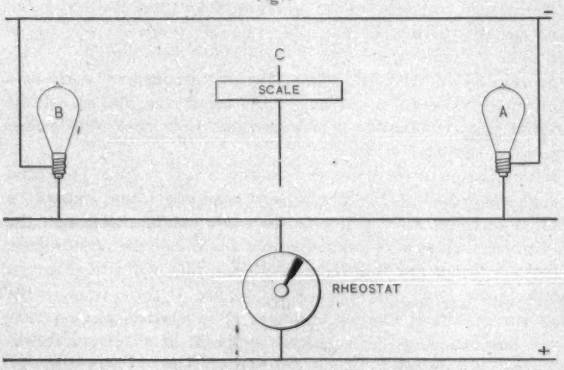
Fig. 1 is a simple diagram showing the electrical connections of a photometer.

A is the working standard, or lamp of known voltage. B is the lamp to be tested, and C is the movable screen, the position of which at the balancing point indicates the voltage of the lamp B. The volt scale is made by a combination of the curves of the candle-power scale of a Bunsen photometer and a curve showing the change of candle-power due to change of voltage.

Fig. 2 is a curve of a candle-power scale of a Bunsen photometer. It is derived from the formula:

$$\frac{100}{x^2} = \frac{y}{(100 - x)^2}$$

Fig.1



x is the distance of the screen from the standard lamp, expressed in per cents. of the photometer length, and y is the candle-power expressed in per cents. of the candle-power of the standard lamp.

Fig. 3 is a curve showing the change of candle-power due to change of voltage. The candle-powers and voltages are expressed in per cents. of the candle-power and voltages at 3.1 w.p.c.

Fig. 4 is a curve of the volt scale of a sliding scale photometer. The voltages are expressed in per cents. of the voltage of the standard lamp, and the distances from the standard lamp in per

cents. of the photometer length. This curve is derived from the curves of Figs. 2 and 3.

For instance: A lamp with a voltage 105 per cent. of the voltage of the standard lamp will, when burned at the voltage of the standard, be burning at 95.24 per cent. of its true voltage. In Fig. 3 the percentage candle-power corresponding to 95.24 per cent. voltage is 71.2 per cent., and in Fig. 2, the per cent. distance corresponding to 71.2 per cent is 54.28 per cent; so that the distance corresponding to 105 per cent. volts is 54.28 per cent. The

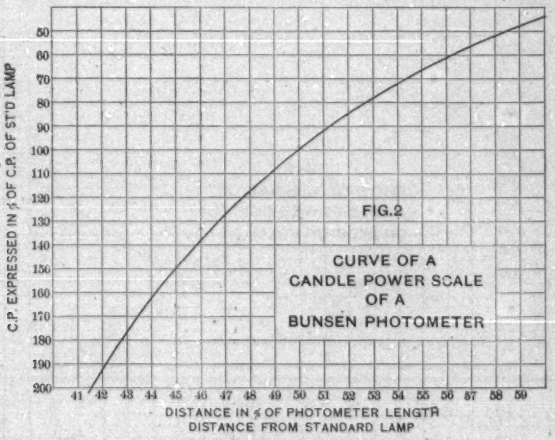


FIG.2

CURVE OF A CANDLE POWER SCALE OF A BUNSEN PHOTOMETER

projection of the integral per cent. volt points of the curve on the length scale gives the volt scale.

The length of the volt divisions for any length photometer and voltage of standard lamp will be directly as the length of the photometer, and inversely as the voltage of the standard lamp.

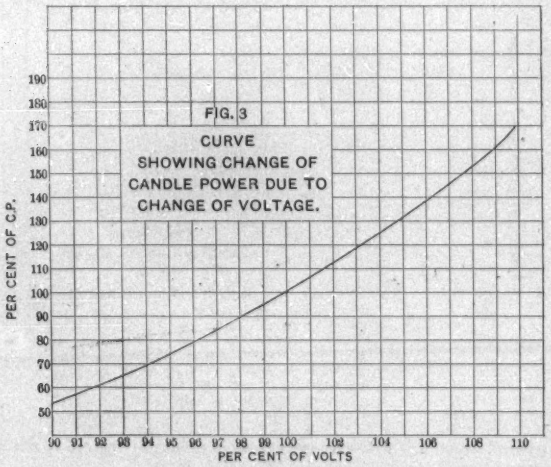
There is a source of error in the use of the sliding scale photometer, in that the curve in Fig. 3 is not the same for all lamps. This difference is due to the fact that the law of change of resistance of filaments with change of temperature or candle-power varies—depending on how much of the filament is base carbon and how much is treatment.

Fig. 5 shows change of candle-power due to change of voltage

for lamps made with filaments of only base carbon, and filaments which have been treated respectively to 75, 50, 37½ and 25 per cent. of the resistance of the base carbon. The voltages and candle-powers are expressed in per cents. of the voltage and candle-power at 3.1 w.p.c.

Fig. 6 shows the volt scales derived from each of the curves referred to in Fig. 5.

The curves of Figs. 3 and 4 are derived from the lamps treated to 37½ per cent. of the initial resistance. This is about an average treatment.



It will be seen in general that the volt scale is contracted more and more as the treatment on the filament thickens. If any one of these scales is used for reading variously treated filaments, it will be necessary to restrict the readings to a few volts either side of the middle point.

These scales were derived from a few lamps, made and measured since I was asked to prepare this paper. Measurements made on a larger number of lamps would probably modify these scales somewhat. The scale actually in use at our factory is shown on Fig. 6 by round dots.

In using the sliding scale photometer it is the practice to select a scale whose middle voltage is very nearly the average voltage of the lamps to be tested; to select for a working standard a well-seasoned lamp of the same candle-power as the lamps to be tested and of a slightly lower voltage than the middle point of the scale; to have a few ohms resistance in series with the working standard, so that its candle-power may be reduced or slightly changed; to read only a few volts either side of the middle point

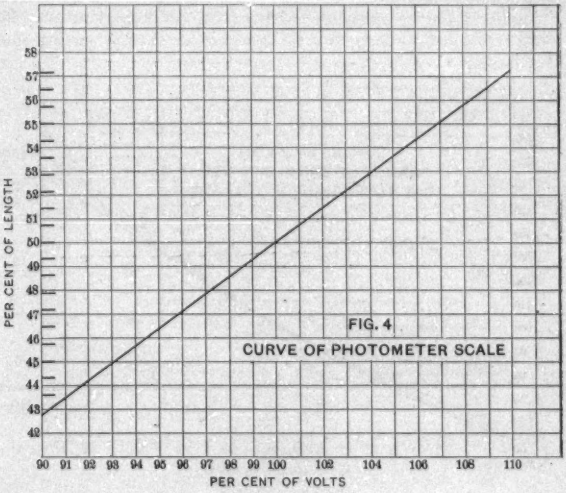


FIG. 4
CURVE OF PHOTOMETER SCALE

of the scale, and to keep the voltage of the line nearly the same as the middle point of the scale.

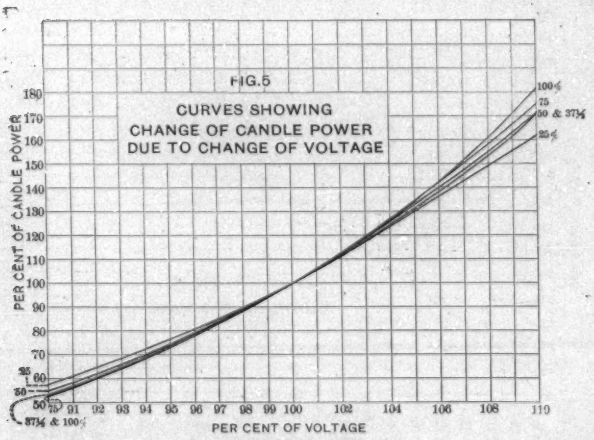
The photometer is set with a few lamps like those to be tested. They are first carefully seasoned and tested for voltage on a standard photometer, and kept for reference lamps. One of them is placed on the working end of the photometer, the screen is placed on the scale at the voltage marked on the reference lamp and the working standard is brought to a candle-power which balances the screen. The other reference lamps are then placed successively on the working end and their voltage read on the

scale by moving the screen. If the observed voltage averages the same as those marked on the reference lamps, the photometer is correctly set.

Attention has been called to the fact that the curves of candle-power and voltage are different for differently treated filaments. It is interesting to note that if curves were made showing the change of candle-power with w.p.c., they would be coincident for all the filaments of Fig. 5.

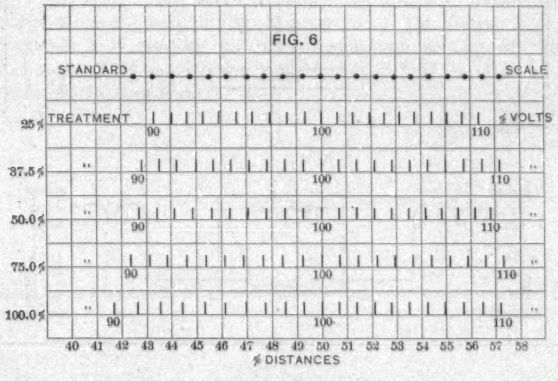
A brief account of the invention and early use of the sliding scale photometer may be interesting.

Some time in the early eighties, Mr. William Holzer, then



Superintendent of the Edison Lamp Company, sent to the life test photometer room several thousand 16 candle-power lamps, from which he asked us to select all that were above 115 volts. I began with the usual method of bringing the lamps up to candle-power and reading the volts with a Thompson galvanometer. Then it occurred to me that I could put in a standard lamp of 115 volts, connect it in multiple with the other lamp, and that all the lamps that were less than 16 candles would be more than 115 volts. The varying candle-powers of the successive lamps put on the photometer suggested the making of the volt scale; and, recalling that Mr. J. W. Howell had in his table drawer curves

like those in Figs. 2 and 3, I made a scale like that in Fig. 4, and the next day tested 1,500 lamps in four hours, something heretofore unprecendented in lamp testing. We were then making so few lamps that it did not seem worth while to change our regular photometer room to this method, particularly as the photometer required resetting for every different candle-power lamp. Mr. F. R. Upton, then General Manager of the lamp company, advised me to patent the invention, but I thought a publication of the method would only cause our competitors to appropriate it without compensation, and therefore did not do so. Soon afterwards, I was sent to our Canadian factory to put the lamp testing apparatus in order. I there found that the engine



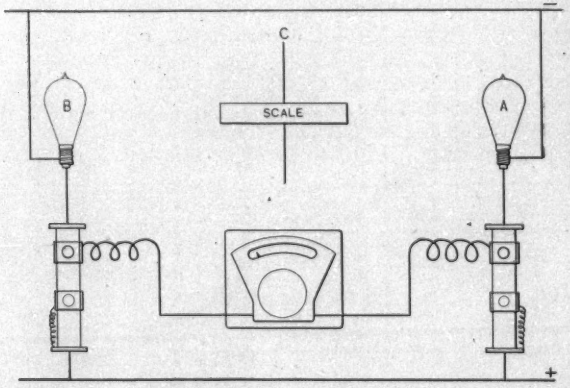
regulation was so bad that the best way to obtain steady voltage was to turn the engine throttle while watching the voltmeter. I found that I could duplicate my results better with the sliding scale photometer, letting the engine race, than by trying to control it and read volts with a voltmeter. I therefore installed the sliding scale photometer and made up a full set of reference lamps After this had been running for a year we commenced to install them in the Harrison factory, and are still using them.

We are using for inspection purposes a photometer connected as per Fig. 7. We call this the differential photometer.

The volt meter reads the difference in voltage of the two

lamps, and has its point of no difference at the middle of the scale, which is marked with the voltage of the standard lamp. The scale is calibrated only a few volts either side of the middle point so that it is very open. One terminal of the volt meter can be slid along the resistance in series with the standard lamp, so as to make the voltmeter read the voltage of the reference lamp used in setting the photometer. The advantage of this type of photometer over the sliding scale photometer for inspection is, that by changing the position of the screen it can be used with the same setting for lamps of all candle-powers, that come

Fig. 7



in the range of the voltage of the meter. The advantage over a standard photometer which reads the total voltage of the lamp, is that a more open scale can be used and changes in the voltage of the line produce smaller errors. For instance, if the standard lamp and the test lamp are the same voltage, a change in the line will make no change in the reading. But suppose that the test lamp is 100 volts, and that the line drops 10 per cent.; then the 10 volts difference will appear as 9 volts, and the lamp will read 109 volts instead of 110 volts, an error of less than 1 per cent., due to change of 10 per cent. in the line.

Our method of inspection with this photometer is to bring the test lamp to the volts marked on it and read the candle-power.

In this way a few lamps are inspected from about a third of all trays of lamps tested.

Mr. J. W. Howell first used the differential photometer as a means of testing lamps for volts, some time in 1897. We are now experimenting with it again for that purpose, and may possibly substitute it for the sliding scale photometer as a means of overcoming the error due to the different volt curves of differently treated lamps.

We are also experimenting on a photometer, using alternating current in a transformer having single volt steps. The test lamp will be brought to its rated candle-power by connecting it to the proper step, and its volts will be shown by the number of steps between the connections of the test lamp and the standard lamp. This method will have the triple advantage of dispensing with a voltmeter, overcoming the errors of the sliding scale photometer, and reading all lamps with one setting of the photometer. This method was suggested by Mr. H. B. Coolidge, of the Lamp Testing Bureau.

A paper presented at the 169th Meeting of the American Institute of Electrical Engineers, New York, Oct. 24th, 1902.

THE COMMERCIAL ACCURACY OF PHOTOMETRICAL MEASUREMENTS.

BY CLAYTON H. SHARP.

The term "commercial accuracy" has a significance which recognizes the fact that for the practical purposes of the engineer and the manager, measurements, tests and computations of all sorts need not be carried out to the highest degree of refinement. This is because variations in working conditions are so large that they mask the effect of ultra refinements in the subject matter of the test, or because the residue neglected in making rougher tests is not sufficiently large to make it pay to expend on its determination the necessary time and money.

It would ,for example, be superfluous to check the accuracy of a switch-board instrument to one-tenth per cent. (0.1%), or to determine the dielectric losses in a cable to a similar degree of accuracy. It is not possible to fix any value for the limits of error within which tests or measurements may be taken as commercially accurate, since their limits vary widely with the conditions of the test. In the case of the switchboard instrument cited above, one-half of one per cent. may be taken as the allowable limit, while in the case of the cable ten per cent. error would not be excessive. A recording watt-hour meter is considered as commercially accurate if it registers correctly within two per cent.

It is probable that in almost any test involving the making of more than one measurement under conditions of fluctuating voltage or current such as nearly always prevail in practice, the result can be considered as commercially accurate if it is within 2 per cent. This value of 2 per cent. may consequently be taken as a kind of general value for the limit of commercial accuracy.

It is the purpose of this paper to inquire whether photometrical measurements conform in general to the requirements of commercial accuracy; that is, whether they stand on equal or nearly equal footing in this regard with other electro-technical measurements. The excuse for presenting this inquiry is the fact that there seems to be a widespread feeling that such is not the case. The idea is sometimes expressed that photometrical measurements are subject to an uncertainty and unreliability all their own, and that their results are often worthy of little confidence. It is undoubtedly true that a certain amount of skill and practice is required to make good photometrical measurements and that certain precautions must be taken, the neglect of which will vitiate such measurements. But it is the business of a photometrist to have such skill and to be familiar with such precautions, just as it is the business of the alternating current engineer to understand the conditions of his work and the pitfalls which are liable to entrap him. The fact that many bad photometrical measurements have been made by persons not sufficiently familiar with the nature of the work, does not argue that this class of measurements is *per se* unreliable.

Photometrical measurements may be divided into three general classes, corresponding to similar classes in electro-technical measurements in general. For these classes, different degrees of accuracy are to be expected. The first class includes those measurements in which the primary standard or unit of luminous intensity is used to calibrate secondary standards of the same. This is similar to the calibration of secondary standards of resistance from a mercury ohm, and like the latter, the results are involved in the uncertainty connected with the production of the primary standard.

The second class comprises the calibration of working secondary standards by reference to the original secondary standards. This is the class of work which can be carried out to the highest degree of accuracy. The third class includes commercial measurements of sources of light by reference to working secondary standards, and corresponds to ordinary electrical testing where direct reading instruments are used and no effort is made toward great refinement. Speed is an important factor in this kind of work.

In order to ascertain if a good commercial accuracy is attainable in photometrical work of these three classes, series of ex-

periments have been carried out in the Lamp Testing Bureau as outlined below.

In testing the accuracy of work of the first class, starting out, that is, from a primary standard, two Hefner amylacetate lamps and one Harcourt ten-candle power pentane lamp were repeatedly compared with invariable incandescent lamp standards.

The Hefner lamps used were manufactured by different German makers, but both bore the stamp of the Reichsanstalt, the one No. 242, dated 1896, and the other, No. 937, dated 1901. These lamps were not identical in all their dimensions. One of them was supplied with a woven wick, the other with a wick of loose strands. The pentane lamp was made by the American Meter Company. The proper burning fluid was used for all the lamps. These lamps represent the best types of primary photometrical standards in use to-day. The proper precautions in their use were taken and the necessary corrections for atmospheric humidity were applied. It was assumed that the pentane lamp required the same percentage correction for humidity as the amylacetate lamp, since the true value for its correction has not been determined by experiment. The measurements were carried out chiefly by Miss Leiter and Miss Merriman who, while both college graduates and trained in laboratory methods, had had no previous experience in handling these lamps. Carried out under these conditions, it is safe to assume that the results obtained can be duplicated anywhere where atmospheric conditions are not excessively bad. The results are contained in Tables I and II.

Inspecting the column of Table I, where the differences between the lamps are given, it will be seen that in two cases these reached the excessive value of 5.2 per cent. and 3.9 per cent., indicating that one of the lamps was not burning under normal conditions. The mean difference between the lamps was but 1.2 per cent. in a series of fourteen measurements made on ten different days. The mean deviation of the values uncorrected for humidity from the mean of all was 1.7 per cent. for both lamps, while the corrected values differed only 1.1 per cent and 1.0 per cent. on the average. The greatest deviation of a single measurement from the mean of all was but 4.8 per cent., but this occurred when the indications of the lamp disagreed by 5.2 per cent., and in any practical case this measurement would have to be rejected. If the mean indication of the pair of lamps is

TABLE I.

HEFNER LAMP MEASUREMENTS

Date.	C.P. uncorrected for humidity.		C.P. corrected for humidity.		Difference between the lamps.	Amount of humidity correction.	Deviation from mean of all uncorrected.		Deviation from mean of all corrected.		Deviations of mean of pairs from mean of all.
1902.	No. 242	No. 937.	No. 242.	No. 937.			No. 242.	No. 937.	No. 242.	No. 937.	
Oct. 8	.880	.860	.903	.883	2.2%	2.7%	−0.5%	−2.6%	+1.3%	−0.7%	+0.3%
" 9	.834	.867	.856	.889	3.9%	2.5%	−6.1%	−1.8%	−3.4%	−0.1%	−1.7%
" 10	.889	.880	.898	.889	1.0%	1.0%	+0.4%	−0.3%	+0.8%	−0.1%	+0.3%
" 10	.894	.893	.905	.903	0.1%	1.2%	+1.0%	+1.1%	+0.5%	+0.3%	+1.4%
" 11	.881	.883	.898	.900	0.2%	2.0%	−0.4%	0.0%	+0.8%	+1.0%	+0.1%
" 13	.888	.841	.889	.842	5.2%	0.2%	+0.3%	−4.7%	−0.1%	−4.8%	−2.5%
" 14	.882	.891	.890	.899	1.0%	1.0%	−0.3%	+0.9%	0.0%	+0.9%	+0.5%
" 14	.866	.881	.879	.894	1.7%	1.5%	−2.1%	−0.4%	−1.1%	+0.4%	−0.3%
" 15	.860	.860	.868	.868	0.0%	1.0%	−2.9%	−2.6%	−2.2%	−2.2%	−2.2%
" 16	.886	.887	.886	.887	0.1%	0.0%	+0.1%	+0.4%	−0.4%	−0.3%	−0.4%
" 16	.894	.887	.894	.887	0.7%	1.5%	+1.0%	+0.4%	+0.4%	−0.3%	0.0%
" 17	.917	.907	.910	.900	1.0%	−0.75%	+3.6%	+2.7%	+2.0%	+1.0%	+1.5%
" 17	.913	.912	.906	.905	0.1%	−0.75%	+3.1%	+3.2%	+1.6%	+1.5%	+1.5%
" 18	.905	.906	.905	.906	0.1%	0.0%	+2.2%	+2.6%	+1.5%	+1.6%	+1.6%
Mean	.885	.883	.892	.889	1.2%	1.1%	±1.7%	±1.7%	±1.1%	±1.0%	±1.0%

taken as representing a measurement, the mean deviation was 1.0 per cent., and the maximum 2.5 per cent.

The secondary standard lamps to which these measurements go for the value of the British candle power are the progeny of lamps standardized at the Reichsanstalt over ten years ago, using the ratio I Hefner unit = 0.88 c. p. At that time the correction for humidity had not been determined by the Reichsanstalt authorities. We find now that in terms of these secondary standards the Hefner uncorrected, equals 0.884 c. p., a deviation of less than one-half of 1 per cent.

The pentane lamp used was known to be an imperfect one, not giving its full 10 c. p., hence too much stress should not be laid upon the low value which it shows. The results in Table II. in-

TABLE II.

PENTANE LAMP MEASUREMENTS.

Date.	C.P. uncorrected for humidity.	C.P. corrected for humidity.	Amount of humidity correction.	Deviation from mean of all, uncorrected.	Deviation from mean of all, corrected.
1902.					
Oct. 8	9.20	9.44	2.7%	−1.2%	+0.4%
" 9	9.00	9.22	2.5%	−3.3%	−1.8%
" 9	9.20	9.43	2.5%	−1.2%	+0.3%
" 10	9.31	9.40	1.0%	0.0%	0.0%
" 10	9.15	9.24	1.0%	−1.7%	−1.7%
" 11	9.33	9.51	2.0%	+0.2%	+1.1%
" 14	9.30	9.39	1.0%	+0.1%	−0.1%
" 14	9.33	9.46	1.5%	+0.2%	+0.6%
" 15	9.11	9.02	1.0%	−2.1%	−4.0%
" 16	9.37	9.37	0.0%	+0.6%	−0.3%
" 16	9.27	9.42	1.5%	−0.4%	+0.2%
" 17	9.67	9.60	−0.75%	+3.0%	+2.1%
" 17	9.76	9.68	−0.75%	+4.8%	+2.9%
" 18	9.40	9.40	0.0%	+0.9%	0.0%
Mean	9.31	9.40	2.2%	±1.4%	±1.1%

dicate that it is at least as consistent with itself as the Hefner. The maximum deviation from the mean was 4.8 per cent. for the uncorrected readings and 4.05 per cent. for the corrected ones. The mean deviations were 1.4 per cent. and 1.1 per cent. respectively, or substantially the same as for the Hefner. It is a fair conclusion from the foregoing that it is feasible to determine luminous intensities absolutely with reference directly to a primary standard with a degree of accuracy falling within the general limit ascribed above to commercial accuracy.

The results of measurements coming under the second class, namely, the copying of incandescent lamp secondary standards,

are shown in Table III. In it are given various determinations of the voltage at which each of 10 lamps gives 16 c. p. The measurements were made by two different photometer operators on different days, as shown, and have been simply abstracted from the ordinary records of the Lamp Testing Bureau.

TABLE III.

STANDARD LAMP MEASUREMENTS.

Lamp.	E.F. Sept. 25.	M.E.P. Oct. 1.	E.F. Sept. 30.	M.E.P. Oct. 8.	E.F. Oct. 14.	Mean.	Mean deviation from mean.
	Volts.	Volts.	Volts.	Volts.	Volts.	Volts.	
1	108.40	108.43	108.19	108.39	108.37	108.4	0.06%
2	108.75	108.79	108.62	108.91	108.80	108.8	0.06%
3	109.05	108.96	108.82	109.04	109.02	109.0	0.04%
4	109.79	109.82	109.75	109.89	109.86	109.8	0.04%
5	107.82	107.80	107.68	107.75	107.64	107.7	0.04%
6	108.77	108.61	108.61	108.63	108.60	108.6	0.04%
7		108.16	107.96	107.82	107.96	108.0	0.07%
8	110.01	109.90	109.85	109.98	109.91	109.9	0.06%
9	108.50	108.42	108.41	108.52	108.42	108.5	0.05%
10	109.00	108.98	109.00	108.83	108.97	109.0	0.02%

The greatest mean deviation from the mean for any lamp is 0.07 per cent. in voltage, which corresponds to about 0.05 c.p. The final results may be taken as certainly correct within narrower limits than this value. The closeness with which photometrical measurements can be made under these conditions is such as to task the capabilities of the best direct reading voltmeter. The voltmeter acutally used in making these was a Weston laboratory standard. Certainly, for commercial purposes, copies of photometric standards need not be made any more accurately than this table shows that it is possible to make them.

As showing what may be and is done in strictly commercial measurements, class three, the results in Table IV. are of interest. Ten lamps were measured at marked volts on four different photometers by four different photometer teams, under strictly commercial conditions. Each photometer was turning out some 500 lamps a day, measured just as these ten were. Each value given represents a single photometer setting.

The greatest deviation of a single measurement from the mean of the four was 2.5 per cent. The mean of the maximum deviations is but 0.4 per cent. It is particularly important to note that the results obtained by different operators on different photometers are in substantial agreement. The deviations of the candle-power of the individual lamps from 16 are to be

TABLE IV

COMMERCIAL MEASUREMENTS.

Lamp No.	C.P. Photom. No. 1.	C.P. Photom. No. 2.	C.P. Photom. No. 3.	C.P. Photom. No. 4.	Maximum Deviation.
1	16.5	16.4	16.3	16.4	0.6%
2	16.2	16.2	16.2	16.1	0.5%
3	16.3	16.4	16.2	16.4	0.8%
4	16.0	16.15	16.0	15.9	0.6%
5	15.9	16.0	16.0	15.9	0.3%
6	16.6	16.9	16.4	16.2	2.5%
7	16.1	16.2	16.3	16.2	0.6%
8	16.2	16.1	16.0	16.0	0.8%
9	16.3	16.2	16.1	16.1	0.8%
10	15.9	15.9	16.1	16.0	0.8%
Mean	16.20	16.25	16.16	16.12	0.4%

ascribed to the fact that these lamps were marked to integral voltages only.

When photometrical measurements are made on sources of light such as the electric arc, the limits of allowable error must be considerably extended. The difficulties of arc-light photometry have often been ascribed to the difference in color which exists between the light of the arc and that of the comparison source. This is undoubtedly a real and a serious difficulty, but it is not an unsurmountable one. The great trouble in arc-light photometry has been that photometrists have been trying to ascribe a constant value to a varying quantity, namely, the luminous intensity of the arc in a given direction. Just as soon as a rational endeavor is made to measure some moderately constant luminous element of the arc, such as its total luminous flux or its mean spherical intensity, good, consistent results can be obtained, as the experiments of Professor Matthews have amply demonstrated.

The points which it has been the purpose of this paper to emphasize may be summarized as follows:

1. There are primary standards of luminous intensity, which, whatever may be their shortcomings from the point of view of the physicist, are sufficiently accurate for commercial purposes.

2. These standards can be copied with as high a degree of accuracy as is desired.

3. Rapid photometrical measurements can be made with all the accuracy required for commercial purposes.

4. In the case in which photometrical measurements are in the worst repute, namely, in the photometry of the arc, the chief fault lies with the varying nature of the quantity which it is sought to measure rather than with available methods of measuring the same.

DISCUSSION.

Mr. Burnett, after reading his paper, continued as follows: I should like to show the arrangement of these "isophotal" curves, suggested in this paper. If we have here (referring to blackboard), a source of light, the customary method of plotting out its action has been to draw a curve on any arbitrary scale, showing the relative intensity in various directions in a vertical plane. My suggestion is that we consider the actual illumination throughout a plane of the space lighted and indicate the various curves of 1, 2, 3, etc.; or 10, 20, 30, etc., candle meters, the chart would be drawn to a scale of so many feet to the inch. The radial dimensions of these curves will be found to vary as the square root of the intensity of the light source in the corresponding direction, and all the of "isophotal" curves will be found to have the same general shape as the single light intensity curve as I now show, except as changed by shadows and diffusion. It would be at least interesting, if not of actual value, to proceed to chart out these curves in real instances. The only set that I have seen are those of Weber, which are referred to in my paper.

Having then mapped out the illumination throughout a vertical plane, we may show on our chart a surface upon which the light impinges, and draw a curve showing the distribution of the lighting in candle meters along a line of that surface intersected by the plane. A study of this sort of curves will show that the distribution of the lighting is much more uniform than a curve showing the intensity of the light in the various directions, radiating toward corresponding points of the surface.

From these rough curves which I have indicated, we make the deduction, as pointed out by Ryan, that the direction of the maximum intensity is different from and higher than the direction of maximum illumination.

As I show here, my instrument compares the normal lighting of the room falling upon this surface with the lighting on a similar and adjoining surface created by an adjustable lamp, suitable shades being provided to avoid interference between the lights and with the normal size of the pupil of the eye. Such an instrument would measure the illumination at the various points of a room and would furnish data for the construction of such a curve as I have indicated.

THE PRESIDENT:—Gentlemen, we have been presented with an exceptionally excellent array of luminous material, and the general subject is now open for discussion. We have with us this evening Prof. Nichols, a recognized authority on the subject before us. Prof. Nichols will open the discussion.

PROF. EDWARD L. NICHOLS:—Mr. President and Gentlemen: I came to-night to listen rather than to say anything, but I should like to express my admiration for the very admirable and ingenious manner in which Mr. Matthews has worked out the problem of rapid measurements of the mean spherical candle

power, both of the arc lamp and of the incandescent lamp. While it is true, of course, that the use to which a lamp is to be put, as the Chairman has stated in his introductory remarks, determines the question as to the direction in which the light shall flow, there can never be any other general method, I think, of comparing light-giving sources, except that of the mean spherical candle power. Whatever methods we use for our convenience or for special purposes, should after all be referred back to this, and the only reason why it has not come into general use, has, I take it, been that it has hitherto been most laborious and uncertain. Mr. Matthews has shown us how this can now be accomplished rapidly, and I am sure that he deserves the highest thanks of all interested in photometry for his labors in this field.

I should also like to say that I feel very strongly the point that the second reader, Mr. Burnett, made in his paper, with reference to the importance of the study of illumination as distinct from the mere photometric measurements of sources of light. We see growing indications everywhere of a realization of the importance of this side of our subject. I might say that we have in the laboratory at Cornell University made a step toward the practical study of these problems, by setting aside a room for the study of illumination. Dr. Sharp, who read a paper to-night, is the originator of that plan, and the room which he devised is being used. It is a room shaped something like this one, although not quite so large. The walls and ceiling can be draped in any desired way, so that the wall surface can be changed, using white, colored or black draperies. These draperies can be moved aside, and the walls, when the test is an important one, can be kalsomined or painted or coated with any desired pigment, and the floor can likewise be treated. The method which is employed and which I think is likely to yield important results, consists in dividing off the floor into squares of one metre and placing upon the floor at various points, a small white card the illumination upon which is determined in various positions by means of the Weber photometer. There are other devices for doing this, but this was the one that happened to be at our command and it works very well. The same piece of cardboard can then be placed on a movable stand at any desired height above the floor, and the illumination can be explored at a distance of one metre above the floor, two metres above the floor, and so on in detail. Thus the light field of the room can be plotted in detail, very much as one might study the magnetic field of a region, and maps can be made of the same. The lighting of the room is likewise capable of adjustment at will; that is to say, you can use as many lamps of a given kind as may be desired and those can be gropped as may be desired, in a central cluster, or around the walls or in any way, and the resulting illumination can be compared. I think that this is a line of work which will be very instructive to those who have occasion

to deal with artificial lighting, and it is certain that it ought to be carried on, not only in closed spaces of this kind, but likewise out of doors, since the question of the best method of street lighting is almost as important as the best method of lighting rooms. The fact has already been pointed out that our newer sources of artificial light are all too bright to be good for the eye, and I think there is no doubt but what the future of lighting as a fine art will consist, in a great measure, in placing the lamps where they themselves will not be seen, and getting the light from them in such directions and such quantity as may be most desirable for the work in question.

We are in the study of illumination only upon the threshold of exactitude. Dr. Sharp has pointed out that photometric measurements are capable of a considerable degree of precision, but photometry is only the beginning of the science of the study of illumination.

MR. FRANCIS R. UPTON:—The method of testing lamps by a comparison with a lamp in the same circuit, was one that solved many a hard difficulty that was met with in the early days of making incandescent lamps. The fact that Dr. Nichols was the first photometrist of incandescent lamps, is interesting, especially as he has followed that out for so many years. What he describes to-night regarding the study of illumination, is most important. What the light itself tests is nothing. It is what it does. That is what the public are looking at. There is one point about illumination that I have never seen mentioned; that is, the effect of the mass of light in New York City, and the mass of light in Newark, upon the clouds reflected back upon the landscape. At Orange at night, this is now so great that on a cloudy night, all the trees, all the houses, can be distinctly distinguished by the light from the clouds. This illumination is such that in New York harbor on any dark night you can see all the boats on the river and even the waves. I remember distinctly that when the question of putting arc lights on the piers or along the water front was discussed, it was thought that they would interfere with navigation. Now, the arc lights are so many, and the general illumination is so great that navigation is much aided.

THE PRESIDENT:—It is rather surprising to note that although clouds in the day time obscure the general illumination, clouds at night contribute to the illumination of the surrounding country. Mr. L. B. Marks has given a good deal of attention to this general subject.

MR. L. B. MARKS: The photometer of Prof. Matthews was brought to my attention about six months ago, and I at once became very much interested in the method of operation which was described to you by him this evening.

I find on looking over some of the literature relative to the subject of photometry, that in 1897, as I believe Prof. Matthews mentioned, the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS made the following recommendation as to the candle-power

rating of incandescent lamps. The Committee on Units and Standards reports: "Although incandescent lamps are at present rated by their horizontal candle-power, yet, since the only true criterion of the total quantity of light emitted by a lamp, is its mean spherical candle-power, we recommend that the rating of lamps should be based upon their mean spherical power so far as is commercially practicable."

Again, at the Great Barrington Convention of the INSTITUTE this year, a Committee further reported on the same subject in these words: "It is customary in industrial practice at the present time, to rate incandescent lamps upon the basis of their mean horizontal candle-power; but in comparing sources of light in which the relative distribution of luminosity differs considerably, the comparison should be based upon the total quantity of light or total flux of light emitted by each source."

The Committee appointed by the National Electric Light Association to investigate this question reported in 1902 in substance, that while in theory the mean spherical candle-power should be the final test for luminous radiation of incandescent lamps or other lamps, until such candle-power can be determined by a more correct and precise process than is now commercially available, it is best to depend upon the mean horizontal candle-power, etc.

In other words, it may be said that up to the present time we have had no means of commercially determining the mean spherical candle-power of incandescent lamps, that is, of making this measurement with the same accuracy and facility as now obtain in making horizontal candle-power measurements. Prof. Matthews shows us a practical way of obtaining the spherical measurement, and it is for this reason that his integrating photometer appeals to me most strongly.

My friend, Dr. Nichols, has long urged that the mean spherical candle-power unit is the only rational basis for rating incandescent lamps. Curiously, there has been great opposition to the adoption of this rating. At the meeting at Great Barrington, a distinguished representative of one of our leading electric light companies, took the ground that such a rating would be inexpedient for the reason that the people had been educated up to a 16 candle-power standard and that the lighting company would suffer if the lamps were sold on the basis of their mean spherical candle-power, which is really only about 13 c.p.

In this connection, I am reminded of our early experience in enclosed arc lighting. When the enclosed arc lamp first came out it was said that lamps of this type couldn't compete with open arcs because the maximum candle-power of the former was only about half that of the latter, for lamps consuming the same amount of energy. People had been educated up to the 2,000 c.p. standard of the open arc and it was contended that the lower rating of the enclosed arc would seriously handicap its introduction. This contention, it is true, was not without some founda-

tion for it took a few years to convince the people that the so-called 2,000 c.p. lamp was not giving them any more light than the 250 c.p. (mean spherical) lamp.

Now, we have an analogous situation with regard to the incandescent lamp. It is contended that the introduction of the spherical rating will seriously handicap the lamp companies, because the so-called 16 c.p. lamp will be rated at only about 13 c.p. But the spherical rating must come. It is the only true standard by which incandescent lamps can be properly compared, and its adoption will surely benefit the lamp companies as well as the lamp users.

Prof. Matthews has shown you how the instrument may be used for measuring the spherical reduction factor of the lamp, that is, the ratio of the mean spherical candle-power to the mean horizontal candle-power. This measurement is of considerable importance. I may say that some of the large lighting companies now specify in their contracts with the lamp manufacturing companies, that the reduction factor of the lamps supplied, must fall within certain narrow and specified limits. The direct measurement of this reduction factor has heretofore been impossible, but Prof. Matthews, as you have seen, has solved the problem in a very simple way.

It is quite as possible and practicable to rate a lamp according to its flux as according to its horizontal candle-power, and as Prof. Matthews has pointed out, the scale of his photometer may be calibrated either in lumens or in spherical c.p., so that the direct measurement of the total flux may be made with as great accuracy and celerity at the measurement of the horizontal c.p.

In ordinary measurements, as you know, it is necessary to slide the photometer carriage along the bar to obtain a setting. In the instrument we have before us, the photometer disc is fixed. Those who have had experience in making photometric measurements will appreciate the value of this improvement.

It also occurs to me that for making candle-power measurements of the Nernst lamp, in which case it is particularly important to measure the mean hemispherical intensity below the horizontal, an instrument of this kind will be very serviceable.

From a scientific point of view, the ingenious method by which Prof. Matthews has solved the question of the inequalities in the cosine law, is of extreme interest. Kennelly has also grappled with this very problem, and has attempted to solve the question by another method. The results of his efforts are quite well known. Prof. Matthews, however, attacks the subject from an entirely new and original standpoint. By a simple radial adjustment of the mirrors, he corrects not only for error in the cosine law, but also at the same time, for any difference in the coefficient of reflection of the mirrors. What is particularly interesting to me, as one who will be apt to use the instrument commercially, is the fact that the mirrors being once

set, need no further adjustment. For the solution of these various questions, I think Prof. Matthews deserves great credit.

MR. W. S. HOWELL:—The Lamp Testing Bureau some months ago tested one of Dr. Matthews' photometers, and found that its results checked very closely against results obtained by standard methods, so closely indeed, that we are now building one. We intend to use it for spherical measurements.

MR. F. S. SMITH:—Personally, I have been very much interested in Prof. Matthews' instrument, and I think that lamp makers are as much indebted to him as lamp users. I think that everyone who has gone into the matter at all, has appreciated that the existing methods of measuring incandescent lamps have been very unsatisfactory, and that the rating, in line with those measurements, has not been such as could be adapted to the various types. There was a paper here some little time ago which brought out that question very forcibly, and Prof. Matthew's instrument certainly reduces to a very simple matter, the question of the proper determination of the real value of a lamp.

THE PRESIDENT:—We have with us this evening, a gentleman who is the Physicist of the National Bureau of Standards, and is now a member of our Standardization Committee. He is here to-day to meet with that Committee—Dr. Rosa.

DR. EDWARD B. ROSA:—Mr. President, I cannot speak authoritatively on the subject of photometry. I can say, however, that I have listened with great pleasure to the papers that have been given; to the description of this very ingenious and practical photometer and to the account of the very satisfactory accuracy which has been attained in the Lamp Testing Bureau in their photometric work. I hope it will not be long before the Bureau of Standards will be able to contribute something to the subject of photometry. As soon as the new buildings of the Bureau are completed and equipped, work on that line will, I hope, be pushed with commendable energy, but at present, equipment is very moderate. We shall hope in the very near future, as I say, to take up photometry, and to be able to supply photometric standards wherever they are required.

THE PRESIDENT:—The subject is open for discussion, gentlemen. We will be glad to hear from any others. Mr. Rice.

MR. CALVIN W. RICE:—Mr. President, I take considerable pleasure in congratulating Prof. Matthews to-night. I think it is three years ago that I visited him in his laboratory to see the apparatus on which he was then working. There is no doubt of the satisfaction he had in showing me what he had then, and I think it must be the greatest satisfaction to him to be here to-night with the perfected device. I am not sure, but I think he was hoping to present this last June at Great Barrington, and was disappointed. Am I right? And therefore it is the added pleasure of finally succeeding. The remarks that Prof. Nichols made to-night are very interesting. I have a written com-

munication from Mr. A. J. Wurts—and, if possible, I would like to have the Secretary give him credit for the communication—suggesting the Room Photometer. In substance, it was just as Prof. Nichols outlined, as a practical commercial photometer for comparing illumination. If I might interject a request to every gentleman who will give remarks to-night, speaking as a member of the National Electric Light Association Committee on Standards, it will be a great help to the art if some one would suggest a satisfactory primary standard, something more permanent, something more definite, if I am correct in using that expression. We are still in need of an acceptable universal standard, and if anyone could suggest an acceptable universal standard that could be reproduced, I think it would be a great help to the art, and I hope that some one to-night in his remarks, would suggest something for the physicist to employ and develop.

THE PRESIDENT:—While Prof. Matthews was speaking, I recalled the very pleasant hour that I spent with him in his laboratory several years ago, and I was struck then with the thoroughness with which he entered into this work, and his paper this evening has shown how fully this work has been carried out, and how every detail seemingly has been elaborated so that he has a very complete instrument.

I notice Mr. W. J. Hammer. Mr. Hammer has given general attention to this subject.

MR. WILLIAM J. HAMMER:—I am afraid I cannot contribute very much to this subject, Mr. President. I think the suggestion that Dr. Nichols made, and which Mr. Burnett, Mr. Upton and others echoed regarding what the public wants in the way of illumination in any given area is an exceedingly important one, and an idea occurred to me which may not be a practicable one now, but which a little later Cornell University or the Bureau in Washington might be able to carry out, *i. e.*, that in such a room as Prof. Sharp arranged at Cornell, the illumination might be studied by placing selenium cells around the room. We know that all parts of a room have been studied in connection with ventilation and heating problems by placing thermometers in all parts of the room and subsequently plotting the curve showing the circulation of air and variation in temperature.

While I am somewhat familiar with the character of that remarkable substance "selenium," and the manner in which it varies its resistance when exposed to a varying amount of light and also the difficulty often experienced in getting the same results twice from the same selenium cell, and of making it so that it would remain constant, still I believe that we shall ultimately be able by means of such selenium cells to study and plot out curves showing the varying degrees of luminosity in all parts of a room.

I had the pleasure during my recent trip abroad of seeing some very interesting work which had been done by Mr. Ernest

Ruhmer and his father in making the selenium cell much more constant and reliable, and I was very much impressed with the future possibilities of it, after witnessing the very interesting demonstrations made at the Ruhmer laboratory in Berlin.

THE PRESIDENT:—Are there others who desire to take part in the discussion?

MR. MARKS:—I understand that Dr. Stratton, of the National Bureau of Standards, is present.

DR. STRATTON:—I can do no more than say what others have said, that I am very much interested in the papers of the evening, especially that of Dr. Matthews upon his new photometer. As to the work of the National Bureau of Standards, Dr. Rosa has stated our position very well. We hope to be in a position to help you in the very near future and to report actual progress rather than plans. It has given me great pleasure to meet with you this evening.

THE PRESIDENT:—I will now call upon the readers of papers for any further remarks they may have and name them in reverse order—Dr. Sharp.

DR. SHARP:—Mr. President, I wish to speak on just one point, that is, with reference to the suggestion of Mr. Rice about standards. I think it would be very desirable indeed to have a better standard, and I would like if Mr. Rice would state specifically what objection he has to the incandescent lamp as a secondary standard of light. Those who have used incandescent lamps in that way, and their name is legion, have found them very satisfactory, I think, for most purposes of photometry; and it would be hard to imagine anything which will more perfectly fulfill the requirements of a source of light, reproducible anywhere and constant if properly used for a limited amount of use, than the incandescent lamp. I hardly think that it is a fair thing—I don't know that Mr. Rice intended to do anything of the kind—to cast aspersions on the incandescent lamp as a standard, without finding fault with it in a specific manner.

MR. RICE:—I would like to remark that I referred to the primary standards of candle power and not to the secondary standards.

MR. MARSHALL:—Mr. President, I have nothing further to add. It is often said that imitation is the sincerest flattery, and I wish to state that this photometer of Dr. Matthews, has been brought to the attention of the lamp works at Harrison, and that we have thought so favorably of it, that we have obtained permission from Prof. Matthews to build such a photometer, and that we are now building one.

THE PRESIDENT:—You intimated in your paper that it was not necessary to get such permission.

MR. BURNETT:—I am very glad to know that Dr. Nichols has undertaken to investigate these questions at Cornell and I was

surprised that we had not heard from him along that line before now.

The Weber photometer, which he is using, is a thoroughly developed instrument, but its objection is that it chokes off the light which impinges upon the surface, the lighting of which is to be measured, and adjusts it so as to bring it down to the lighting from a standard lamp on a surface at a fixed distance. The same difficulty, if it can properly be called a difficulty, exists with the Houston and Kennelly luminometer. In that instrument the light falls upon a surface of glass, is transmitted through it and is then choked off in a similar way so as to give an illumination equivalent to that of a small standard lamp at a fixed distance.

The object of such an instrument should be not in any way to vary the light which has fallen upon the surface, but to compare our standards with the light and thus get a direct measurement or comparison. I think I can say, without any show of egotism, but purely in the way of reference, that the instrument which I have suggested involves the desirable feature of Prof. Matthews' instrument, that the position of the photometer surfaces by which the lights are compared is not varied and does not require any adjustment of the position of one's head in taking the measurement.

THE PRESIDENT:—I think I may state in a word a résumé of the remarks which have been made upon Prof. Matthews' paper —that he has given us something of exceptional interest and value, his results show careful scientific work, and the embodiment of this work in an instrument which is of practical utility. It is well worked out. He has given us something which is valuable and useful, an advance is made in the art of measurement, and he has also given us an account of his work which is also of particular interest, as it shows an evolution. It is an instance of interesting personal work which has had a very happy outcome in successful results. Prof. Matthews.

PROF. MATTHEWS:—I do not think ,Mr. President, that I have much more to say. I wish to express my hearty appreciation of Dr. Sharp's paper, because I think that photometry and photometric measurements have long been under a cloud. This is partly due to the fact that anybody thinks he can make photometric measurements, if he cannot do anything else—an opinion which is not true. The basis of photometry is a very simple thing, but the practice of it takes skill and experience, and therefore I was very glad to have brought up here some discussion which shows what can be done in good commercial photometry.

Of the many kind remarks that have been made regarding my own work, I wish to express my appreciation. I have put a good many years of work on photometric subjects. I think perhaps the remarks this evening will be inclined to increase the mean spherical radius of my head, if I am at all susceptible.

However, I thank you very much for the kind reception you have given me.

On motion, the meeting adjourned at 10.45 P. M.

[COMMUNICATED AFTER ADJOURNMENT BY EDWARD P. THOMPSON.]

Having followed the invention underlying Prof. Matthews' new photometer, through the United States and foreign Patent Offices, and having been unable to attend the meeting, I take pleasure in adding my commendation of the valuable experimental and practical work obtained by the author of the paper. It may not be amiss to add something about the actual novelty of the device considered as an invention. This may best be accomplished by quoting one of the claims granted by the United States Patent Office as an award for his ingenuity.

"In an apparatus for measuring candle power, the combination with the support for the light to be tested, of a photometer screen, and mirrors so fixed as to illuminate said screen proportionally to the mean spherical candle power of said light, said mirrors remaining stationary during the operation of measuring the light."

This is one of the twenty-three claims granted by the Patent Office. It serves to show that Prof. Matthews has originated a new type of photometer, and the members will no doubt greatly appreciate his services in providing an instrument which will so readily save time and tedious calculation.

I have also examined complete drawings of Mr. Burnett's photometer for measuring illumination in terms of the number of luxes instead of in terms of distance. The device as practially carried out is exceedingly simple, and without movable parts. There is nothing to get out of order.

CRITICISM ON THE MEAN SPHERICAL CANDLE POWER AS A STANDARD FOR ILLUMINATION.

[COMMUNICATED AFTER DISCUSSION BY ALEXANDER J. WURTS.]

The mean spherical candle power is unquestionably the present accepted standard of light, but in reviewing a subject of this kind, it often is instructive to consider whether we may not be somewhat biased by a practice, rule or standard which has ceased to be either useful or practical. The question arises as to whether we wish to continue this standard as our guide. Does the so-called standard meet with our approval? Does the mean spherical candle power, as a standard, measure that which we desire to have measured? In the opinion of the writer, it fails to do so and it would seem that something which more nearly meets our present needs should be devised.

In years gone by, when the incandescent lamp was practically the only electric lamp in commercial use, and later, even after the arc lamp had been introduced, the photometric determination

of candle power served a purpose and gave a nominal rating to these lamps, but the great difference in candle power between the arc and incandescent was such that comparison between them on a basis of mean spherical candle power was of small consequence. With the advent, however, of new illuminants, the question of fair comparison becomes at once a matter of considerable practical importance.

In the laboratory, the photometer has proved itself a valuable guide in determining the relative life and efficiency of illuminants under varying conditions. If there were only one kind of illuminant to be considered, then units differing slightly in candle power could be easily and satisfactorily compared with this instrument, either by direct measurement or by the mean spherical candle power method; but with illuminants differing widely in candle power, quality and distribution of light, neither the direct measurement on the photometer nor the mean spherical candle measurement offer a fair basis of comparison, because two lights having the same mean spherical candle power might differ widely in quality and distribution of light or effective illuminating power. For example, let us consider two lights exactly the same in every respect except that one throws all its light upward, whereas the other throws it all downward. Both have the same mean spherical candle power, but obviously not the same effective illuminating power for a given purpose. Take for further illustration the various well-known electric lights—the incandescent, the arc, the Nernst, the Hewitt and the Bremer. These all differ widely in both quality and effective illuminating powers for a given set of conditions, and as above shown, cannot be consistently compared by the standard of mean spherical candle power.

It follows logically and as a matter of fact that a user of light never, it may be said, inquires about the mean spherical candle power of an illuminant; the only use he makes of a photometer is to test with it some guarantee of candle power which may have been furnished with his lamps. The practical user in negotiating for this or that illuminant is more interested in the effective illuminating power of the lamp in question than he is in the mean spherical candle power, and by effective illumination is meant that amount of light which is reflected to the eye from the objects it is desired to see. The total light flux is of no consequence to the dry goods man or his customers, to the hotel keeper or his guests, except in as far as upward or horizontal rays may be reflected from ceiling and walls. Interest centers rather in (1) the quality of the light; (2) the effective illuminating power; and (3) cost of maintaining the illumination. If this last statement be correct, may not the comparison of various illuminants as to the three points involved be more practically and satisfactorily determined in some other way than by the mean spherical candle power method; that is, by a method which does not place on an equal footing a sphere of soft white

light and a search light, assuming that both have the same mean spherical candle power.

Having thus criticised the mean spherical candle power method of comparing lights, your attention is called to a rather old photometer or means for comparing different illuminants which seems to be worthy of more attention than it has yet received and one which also seems to be capable of considerable development. The photometer in mind provides a means for comparing the effective illuminating powers of various illuminants and involves every characteristic, advantage and disadvantage which may be found in any particular class of illuminants. This method has been used from time to time in a crude way, by placing different kinds of illuminants in the same room and noting the illumination, but could not this idea be carried out on a more practical and elaborate scale by providing two rooms exactly alike in every respect, that is, in dimensions, color and furniture, and then by locating standardized units of light, such as 16 c.p. incandescent lamps, in one room, comparing the effective illuminating power of these lamps as a standard with the effective illuminating power of any other lamp or lamps arranged in the other room in any manner whatsoever which will produce the best results—namely, the most effective illumination with the particular kind of lamp. For example, suppose it be desired to illuminate a dry goods store, the chief points to be considered being the illumination of goods on the counters and on the shelves back of the counters, also boxes and their labels above the shelves to a height of eight feet above the floor. Question: For a given expenditure of energy, can this illumination be obtained more effectively with this or with that lamp. In making such comparison of effective illumination, it would seem proper not to make any restrictions whatever as to the position or distribution of the units. Both the lamps to be used as standard and the lamps to be compared with the standard should be located and distributed to the best possible advantage with reference to effective illumination.

With the two-room photometer above described, it would be a simple matter for the observer to place himself in a position commanding a good view of the two rooms for purposes of comparison. The illumination of the standard room might be easily varied by using a considerable number of small units and the wattmeter readings would of course give the relative efficiencies for equal effective illuminating power. Comparisons of this kind have been made by the writer, not, however, in such an elaborate manner as above described, but if the various illuminants now in the field could be thus authoritatively compared with reference to effective illuminating power and the results tabulated and given to the public, users of artificial light would have before them a reasonable and practicable means of determining the best illuminant for a given set of conditions.

Meeting of the Local Section of the American Institute of Electrical Engineers.

MINUTES OF MEETING HELD AT ELECTRIC CLUB, PITTSBURG, NOVEMBER 10, 1902.

The meeting was called to order by Mr. P. M. Lincoln, Chairman of the Temporary Committee, who read the committee's report. This report advised the election of a committee consisting of a Chairman, a Secretary and three other members, to have charge of the preparation of programs, calling of meetings, etc.

The report was adopted, and the following Permanent Committee was elected: Mr. P. M. Lincoln, *Chairman;* Mr. J. S. Peck, *Secretary;* Mr. H. W. Fisher; Prof. S. M. Kintner; Mr. C. W. Rice.

Prof. Kintner read a paper, giving a resumé of the papers presented at the New York meeting of the A. I. E. E.

Mr. C. F. Scott gave a resumé of the discussion at the New York meeting; also an extremely interesting description of the John Fritz dinner given in New York on November 7th, as well as a brief account of the work of Mr. Fritz and of the founding of the John Fritz medal.

Mr. A. J. Wurts gave a talk on illumination in general, with particular reference to the Nernst lamp. Attention was called to the fact that no illuminant has ever been discarded; candles, oil-lamps, gas and incandescent lamps are all in extensive use. The custom of rating lamps according to mean spherical candle power instead of by effective illumination was severely criticized. The great flexibility in the size of the Nernst lamp was pointed out. It was predicted that the Nernst lamp will have a great influence on street lighting as well as on interior illumination. Mr. Wurts then read a copy of the communication sent by him to the New York meeting.

Mr. R. H. Henderson gave a description of the Bremer flame arc lamp. Attention was called to the fact that the arc lamp was the earliest form of the electric lamp, and that a great amount of experimental work has been done on methods for holding carbons in proper position. The Bremer lamp is strictly a German invention, and no American improvements have been made upon it. The carbons are manufactured according to a secret process, and only in Germany. The most striking features of the Bremer lamp are the composition of the carbons, their position in the lamp, and the fact that the arc is drawn out fan-shaped by means of a magnet placed around the arc. When the carbons are heated, mineral vapors are given off, which become luminous, and give the arc the peculiar flaming appearance as well as its yellow color. The two carbons are nearly parallel, converging at an angle of about 20°. The arc

is started by means of a starting device, which short circuits the ends of the two carbons. The arc is prevented from traveling up between the carbons by means of the magnets which surround the carbon points and hold the arc in position, as well as spreading it out fan shaped. Tests have shown the Bremer lamp to be over three times as economical as the ordinary arc lamp.

Lamps were exhibited and explained in detail.

Dr. von Reckinghousen read the accompanying paper, descriptive of the Cooper-Hewitt lamp. Several lamps were shown in operation, and were frequently referred to by the speaker.

Mr. P. M. Lincoln then talked for a few minutes on the acetylene gas lamp. Attention was called to the fact that although the acetylene light is not an electric light, it is an indirect production of electricity, inasmuch as the electric furnace is the only method yet devised for producing calcium carbide from which the acetylene gas is made. The cost of producing acetylene gas was briefly discussed. The values given are approximately as follows: 1 kw. hour will produce 250 candle-power hours by incandescent lamp, 500 candle power by Nernst lamp, 1,000 candle-power by arc lamp, 2,000 candle-power by Cooper-Hewitt lamp, and 110 candle-power by acetylene. In the production of calcium carbides, however, an electric plant may be operated at full load for 24 hours per day, while the ordinary lighting station does not ordinarily deliver more power than is equivalent to full load for two hours per day. A 25 candle-power acetylene lamp was exhibited, and it was stated that the cost of operating this lamp was not more than one cent per hour.

The following discussion then took place:

Q. Can the Cooper-Hewitt lamp be operated in any position?

A. Yes; provided it is arranged for the position in which it is to operate.

Q. What is the pressure in the Cooper-Hewitt lamp?

A. 2 mm. of mercury when operating. Almost absolute vacuum when cold.

Q. What is the range in size of the Cooper-Hewitt lamp?

A. The smallest size made up to the present time is approximately 20 inches long, giving about 180 candle-power. The longest about 20 feet, giving approximately 3,000 candle-power.

Mr. Edward Bennett called attention to the statement made by the President of the INSTITUTE at the New York meeting to the effect that "quality" of light was of great importance and said that it could not be brought out too strongly. Mr. Bennett discussed at some length the question of the different kinds of light, and the effect of their quality on the human eye.

Mr. N. W. Storer expressed his sympathy for Mr. Wurts in his contention against mean spherical candle-power as a unit of measure, as he was conducting a similar campaign with reference to the one-hour-horse-power rating of street car motors;

but as a horse power rating is demanded by the commercial conditions, so a candle-power rating would also be demanded.

Mr. Wurts stated that the candle power rating does not fill the commercial want, as it is not a unit of measure of effective illumination.

Mr. F. W. Jones stated that from large experience in lighting buildings he was convinced that mean spherical candle power rating was in no way satisfactory, but that effective illumination was the thing demanded.

An animated discussion then took place between Messrs. Wurts, Kintner and Storer, with respect to the necessity of maintaining the candle power rating.

Thirty INSTITUTE men and about 175 visitors were in attendance.

PAPER READ BY DR. VON RECKLINGHAUSEN AT THE PITTSBURG LOCAL MEETING OF THE A. I. E. E., NOV. 10, 1902.

The artificial illuminants which we now use are all based on the principle of the incandescence of solid bodies due to high temperatures. In most illuminants this incandescent body is carbon. For instance, in the incandescent and the arc lamp. It is also carbon in the ordinary gas or kerosene flame. In the latter two the solid carbon is produced in a very fine state, through decomposition of the hydrocarbons at high temperature.

The Cooper-Hewitt lamp, which I have the pleasure of showing you to-night, is the first high candle power illuminant not based on an incandescent solid, but on an incandescent gas. This incandescence takes place under the influence of the electric current. Many attempts have been made to produce light by means of the action of electric currents passing through gases or vapors, especially when the latter were under low tension. I recall the Geissler tubes and the Moore vacuum tubes. With very few exceptions these methods were based on electric currents of entirely different type from the kinds we use commercially. The earlier workers in this field rarely succeeded in getting high candle powers. None of them went so far as to introduce their illuminants into commercial use.

The particular type of Cooper-Hewitt lamp I am showing you is the mercury vapor lamp. The mercury vapor is enclosed in glass tubes, and when the lamps are cold there is practically a vacuum in the tubes. When they are running normally the pressure is about 2 millimeters. The electrodes we use are mercury as negative and mercury or iron as positive. The connections of the electrodes are made of platinum wires sealed into the ends of the tubes. The lamps, as you see them, are running on the ordinary D. C. street current of 120 volts, and they consume about 3 amperes each. The voltage across the terminals is about 80, and the candle power is, as nearly as we can measure, 700.

You will see readily that this means an extremely high efficiency; as a matter of fact, we get about ten times the light from the same amount of energy that we get in incandescent lamps. Lamps of about the same type are in continual use in our factory, where they have been for several months, and show little or no depreciation.

We have made lamps of other shapes and dimensions. We find that the voltage is proportional to the length under otherwise similar conditions. The main difference in conductivity between these lamps, and metallic conductors is that an increase of current within certain limits will not be accompanied by an increase of voltage. I suppose you have seen that there is a certain peculiarity about the way I started the lamps. When they are put right on the line voltage no current will pass through them. We have to make them conductors first. This we do by means of a single discharge of very high voltage, upon the passage of which the lamp becomes a very good conductor, and a low pressure will permit heavy current to continue. If we put this type of lamp on an ordinary alternating current circuit, we have to start it at every alternation, which would practically mean running it continually on the very high starting potential. To pass 2 amperes through the lamp we would have to use about 4,000 volts, while the lamp runs on D. C. at about 100 volts. The seat of reluctance to starting seems to be in the negative electrode, and this reluctance is overcome as described, though when starting the lamp we considerably increase the effect of the high potential kick, by using what we call a starting band; that is, a metal strip outside the negative electrode, connected by a wire to the positive electrode.

A peculiar phenomenon can be seen at the point where the the current leaves the negative electrode and goes over to the vapor. A very brilliant spot of light marks this space on the mercury surface and causes a certain amount of flickering near the negative electrode, We can prevent this by having a small wire projecting out from the surface of the mercury. To this wire the light spot will attach itself. Above this light spot for about an inch there is a fairly dark space in the tube, and then the light-giving column starts. There are also other very interesting phenomena noticeable near the negative electrode, especially under the influence of a magnetic field. One of the most striking features of the mercury vapor lamp, as you will notice, is the strange color and the strange way that things look within the range of the lamp. This color is peculiar to the kind of vapor. We have been making lamps with other gases than mercury, giving other colors; but as we had to take certain disadvantages in exchange, we preferred, for the time being, to stick to mercury vapor as our conducting gas.

It is only natural that many people who see this lamp for the first time ask why we don't use colored glass to get some red into the light. A simple look at the lamp through red glass, or

observation of the spectrum of the lamp, shows that we cannot get red rays from an illuminant that does not send out any. The result of using a red glass is to kill every part of a spectrum except red. It means, in our case, hiding the light entirely. Another way which one might propose to obviate the ghastly effect of the mercury light is to use red reflectors, but you will readily believe that this is just as hopeless as using red glass or globes. There is still one way, which we have tried, as you will see, with some success; that is, using a fluorescent screen, coated in a peculiar way, or else silk dyed in a certain way. The peculiar dye which we have used in these cases has the property of fluorescing red under the influence of the mercury light; that is, it transforms some of the wave lengths into red waves. Still, we think that without these artificial means of improving the color there is quite a large field for these lamps, especially where color is of no account. One very interesting feature of the Cooper-Hewitt lamp is its actinic power. Its effect on photographic plates is extremely strong, and the lamp is on this account useful in photographic studios, and especially for printing purposes. We have been taking photographs with the Cooper-Hewitt light with about 1-30th the exposure that is required with an arc light consuming the same amount of current.

I would also mention one more extremely important fact. We find that the Cooper-Hewitt light is far less tiring to the eyes than any other kind of illuminant. This is not only our own experience, but it is also confirmed by our mechanics and draughtsmen and other people who have been using it continuously for some time.

Everyone knows what a great strain on the eyes is draughting and other delicate work in artificial light. It seems that we have found in the Cooper-Hewitt lamp an illuminant which will do away with these troubles.

A paper presented at the 19th Annual Convention of the American Institute of Electrical Engineers, Great Barrington, Mass., June 21st, 1902.

A VARIABLE RELUCTANCE METHOD OF MOTOR SPEED CONTROL.

BY G. FREDERICK PACKARD.

On August 22, 1882, a patent was issued to Thomas A. Edison on a regulator for dynamo or magneto-electric machines. This patent illustrates and describes the employment of a block of magnetic material of conical form, adjustable in a conical opening in the back yoke of the field magnet. The patent says:

"I make use of the principle that the power of an electro-magnet can be weakened by diminishing the mass of the yoke connecting the cores, and that such power can be strengthened by increasing the mass of said yoke until the maximum power is attained. In applying this principle, I provide the yoke of the field of force magnet of a dynamo or magneto-electric machine with a movable portion, the position of which can be varied so as to effect the mass and conducting power of such yoke, said movable portion acting as a magnetic circuit regulator."

In this patent the movable portion is shown and described as adjustable either by hand or automatically, by a solenoid located in the circuit.

Fig. 1 is a copy of this patent drawing. By means of the screw threaded hand wheel it will be seen that the conical block G is raised and lowered within the conical opening of the yoke causing the block to include more or less of the flux in accordance with its vertical position. By making this block of large diameter, relatively to the width of the yoke, it is evident that the reluctance of the magnetic circuit can be varied within very wide limits.

As far as the writer is aware, this is the first patent that was ever issued, covering adjustable means for governing the field

strength in a dynamo-electric machine by varying the reluctance of the magnetic circuit.

Three years later a patent was issued to Philip Diehl on a dynamo-electric machine the principal novel feature of which was the means for varying the length of the air gap, the two limbs of the bi-polar field magnet being connected by a hinge joint.

In the Edison method, the total reluctance of the magnetic circuit is varied by varying the reluctance of the iron portion thereof, while in the latter case, the length and therefore the reluctance of the air path is varied.

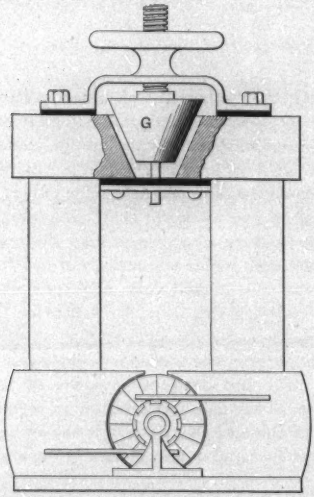


Fig. 1

It may be said in favor of these two variable reluctance methods that, within their limits, they afford an absolutely continuous range of variation, and that the controlling means are so simple and rugged as to make derangement almost impossible.

These methods, however, have never been employed to any great extent for the reason, doubtless, that for ordinary purposes they offer no important advantage over the shunt rheostat method of field control.

Considering the foregoing methods, whether we place a variable resistance in the field circuit or a variable reluctance in the magnetic circuit, the object sought is practically the same, namely, to produce changes in the volume of the flux acting on the armature.

The efficiency of the two methods is practically identical, the use of the rheostat securing a slight saving of field current as compared with the variable reluctance method which employs a constant m.m.f.

In the case of generators, in which the field strength is controlled from a switch board, the use of a rheostat in the field circuit is, of course, the only feasible method.

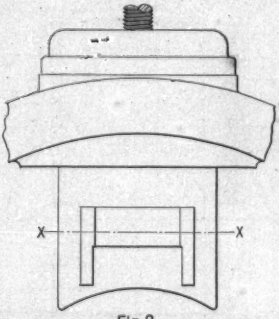


Fig.2

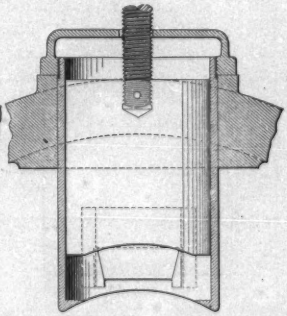


Fig.3

With motors the case is different. While it is true that the speed for railway, elevator, traveling crane motors and motors for some other purposes is of necessity controlled from a distance, in the majority of other motor applications the speed controlling apparatus may, without inconvenience, be placed on the motor or become a part of it.

It will be seen, therefore, that the variable reluctance methods before cited and the use of the shunt rheostat present about equal advantages in many motor applications in which only a small range of variation is required. For controlling the speed of motors, the chief drawback in all the methods referred to is the narrowness of speed range within which the motor may be operated without prohibitive sparking, this range extending in a well-designed motor from the normal speed to about one-third higher than normal speed.

Comparing the Edison bi-polar motor arranged as in the Edison patent cited, with a standard Edison motor of same size whose field strength is controlled by a field rheostat, it will be seen that, with a given speed and load, the conditions which effect the sparking are the same in both cases; in other words, the distribution of the magnetic flux at the pole faces of the two machines will be substantially identical.

Since the sparking is so noticeably effected by the magnetic distribution at the pole faces, it may be well to recall some of the conditions upon which successful commutation depends. Imagine a drum armature coil, the two ends of which are connected to commutator segments A and B. After sweeping across the pole

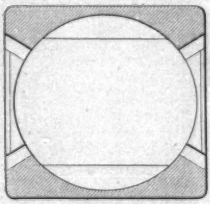


Fig. 4

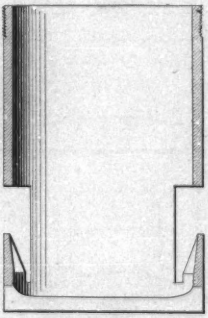


Fig. 5

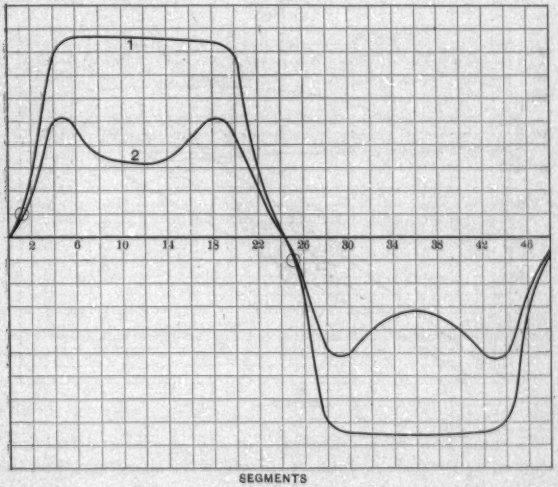
faces and contributing its electromotive force to the circuit, this coil arrives at the point of maximum enclosure of flux, or the neutral position, in which no e.m.f. is induced in the coil.

Although the coil is momentarily idle, it carries all the current of that part of the armature circuit to which it belongs. Now suppose that, an instant later, the segments A and B, at the ends of the coil, pass under a brush. As a consequence of the property of self-induction the current in the coil thus short circuited by the brush cannot immediately be stopped, but requires time. In order to stop the current as rapidly as possible, and to start a reverse current in the coil during short circuit, it is found necessary to give the brush such a lead beyond the point of maximum e.m.f., that while the coil is being short-circuited, it shall have a small reverse e.m.f. induced in it, by cutting into

the magnetic fringe of the adjacent pole piece. The pole tips toward which the brushes are displaced must, therefore, throw out a fringe of sufficient intensity to reverse properly the maximum current that flows in the short-circuited coil.

Consider now the case of a shunt motor of ordinary design whose speed it is required to vary by a resistance in the field circuit. By thus weakening the field the magnetic densities in the various portions of the pole face are reduced uniformly so that although we reduce the volume of the flux, the relative distribution is practically unaltered.

Fig. 6



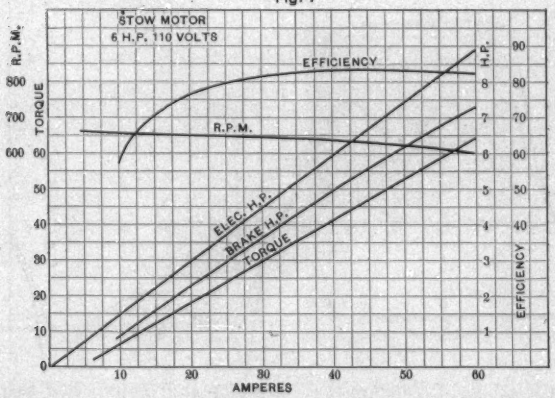
If this machine were economically designed for operation at normal speed, by reducing the field current a point will soon be reached when the fringe at the pole tips will be inadequate to accomplish sparkless commutation at full load.

The exact percentage of speed variation obtainable by the use of field resistance is evidently different in different machines having the same nominal rating, the amount of variation being governed by the armature reaction, the air gap density, the number of commutator sections between adjacent brushes of

opposite polarity, and other points of design; but the writer believes that 33 per cent. is generally accepted as about the limit within which it is feasible to increase the speed of a motor by the use of resistance in the shunt circuit.

Referring back to the two variable reluctance methods devised by Edison and Diehl, we find that the weakening of the field strength by either of these methods does not materially affect the relative distribution of the flux over the pole faces; in consequence of which these methods are subject to precisely the same limitation in the matter of range of speed as is the method in which a field rheostat is used.

Fig. 7



We come now to a description of the special construction patented by Frederic A. Johnson, in which the speed variation in machines of 10 h.p. and over is fully four times as great as can be obtained by field resistance or by the variable reluctance methods herein before described.

We have seen that in the ordinary field regulated shunt motor, the speed cannot be increased beyond certain narrow limits, on account of the inadequacy of the fringe at the pole tips. It is, therefore, clear that if means are employed by which the flux at the pole tips is maintained practically constant, irrespective of changes in the total induction in the pole piece, then the speed may be materially increased, without causing sparking at the

commutator. This result is secured in the motors built under the Johnson patents, as will be shown later from the result of tests. No essential difference exists between the special motor herein described and those of ordinary design, except in the construction of the pole piece which is made hollow, and within which is a solid cylinder of soft iron, radially adjustable. When adjusted to its innermost position, this cylinder or "plunger," as it is called, extends through the inner end of the pole piece, thus form-

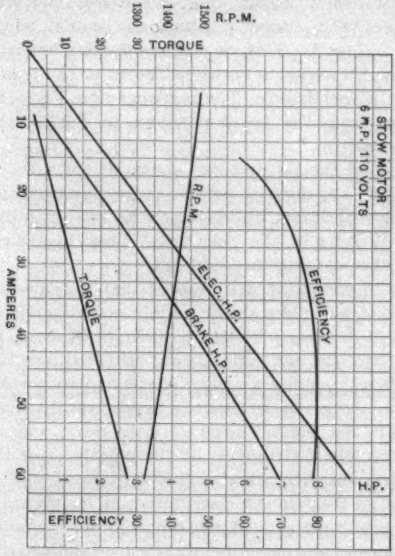


Fig. 8.

ing a portion of the polar surface, and as its cross section is much greater and its permeability higher than the inductive section of the pole piece, it carries the larger portion of the flux.

With the plunger in this position, the magnetic circuit is most complete and of minimum reluctance, and, since the m.m.f. of the field coil is constant, the volume of magnetic flux becomes a maximum and the speed minimum. When the plunger reaches the limit of its outward movement the reluctance of the magnetic

circiut and hence the speed becomes maximum. Fig. 2 is a full view of the pole piece properly secured in the magnetic yoke, while Fig. 3 is a corresponding sectional view showing the inner construction.

As shown by Fig. 2, two opposite sides of the pole piece are cut away, leaving the sides solid over the pole tips. From Figs. 2 and 3 it will be seen that when the plunger occupies its outermost position the entire flux must reach the armature by way of the pole tips, thus affording the fringe at the pole tips necessary for sparkless commutation. Fig. 4 is a sectional plan view of the pole piece with plunger removed, the section being taken on the line x, x, Fig. 2. This view shows the two opposite sections through which the flux is conveyed down to the pole tips when the plunger is withdrawn. The sectional view, Fig. 5, is taken at right angles from the position shown in Fig. 3, the plunger being removed.

The object of the tapered lugs shown at the sides of the pole piece in Fig. 5 is to divert a portion of the flux from the plunger, as it is being withdrawn, and being connected integrally with the two opposite sides of the pole piece, these lugs also serve to strengthen the pole piece mechanically.

From the foregoing it will be seen that the operation of the special form of pole piece shown and described herein, is to force a gradually increasing proportion of the total magnetism down to the pole tips, the entire flux being delivered directly over the pole tips when the plunger is raised to its highest position.

In order to demonstrate that the desired results were actually secured with this special form of pole piece, the writer has measured the flux distribution at the pole faces of the various motors by the statical method proposed by Mordey.

Fig. 6 shows the relative distribution of magnetism half way around the armature of the 4 pole 6 h.p. Johnson motor. In this test a 500-turn exploring coil embracing two adjacent teeth was connected to the low reading terminals of a Weston voltmeter, and the throw of the needle observed on suddenly turning off the normal field current.

Observations were made at 49 equidistant points along the polar circumference, the diagram, Fig. 6, being plotted from the readings thus obtained. Two sets of readings were taken.

The outer curve, No. 1, was plotted from the first set of readings, and indicates the distribution when the plungers occupy their innermost positions, that is to say, when the total flux is maximum.

To obtain the inner curve, No. 2, the field strength was reduced to minimum by withdrawing the plungers from the armature as far as possible; the same exciting current was interrupted at the same speed; and the exploring coil made to occupy precisely the same positions within the field as in the first set of readings.

It is thus possible from these two curves to obtain a comparison of the flux distribution under the conditions of maximum and minimum field strength. Curve 1 resembles closely the curve of distribution which might be taken by exploring the field of any direct current motor of ordinary design.

The peaks in Curve 2 indicate a relatively strong magnetic field adjacent to each of the pole tips, while the depression between the peaks is caused by the withdrawal of the central portion of the pole face. In Fig. 6 the distance between adjacent vertical lines represents the width of two commutator bars. To secure non-sparking at all speeds in this machine it is found necessary to give the brushes a permanent backward "lead" from the neutral point of one and a quarter segments, this lead being indicated by the two circles above and below the horizontal axis.

It will be noticed that with a fixed lead of the brushes the strength of the commutating field is different under the conditions of minimum and maximum speed. In fact, it has been found that the machine operates sparklessly at the lowest speed by giving the brushes a lead of only one segment and at the highest speed by giving a lead of one and a half segments. If the brushes be given a permanent lead of one and a quarter segments, the machine carries its load sparklessly both at the lowest and at the highest speed.

From the manner in which Curves 1 and 2 were obtained, they do not accurately indicate the flux distribution when armature reaction is present. It might be supposed that when the field strength was weakest the armature reaction would so distort the field as to necessitate a greater displacement of the brushes from the neutral point than is required when the field strength is maximum. Such a condition is, however, avoided, owing to the particularly fortunate circumstance that as the field is weakened by withdrawing the plungers, an increasing reluctance is interposed in the path of the armature reaction, until when the plungers are fully withdrawn the cross magnetizing effect is reduced to a minimum from want of a conducting path through which to act. These motors are not the first machines in which

the pole pieces have been provided with radial air spaces for the purpose of reducing the effect of armature reaction.

In the pole pieces of some of their multi-polar generators, about a dozen years ago, the Thomson-Houston Electric Co. made use of a wide radial slot, which practically divided the pole pieces in two equal parts, the purposes of this radial slot being to secure a more uniform distribution of flux over the surface of the pole face; to maintain a magnetic fringe at the trailing pole tip of sufficeint intensity to insure sparkless commutation; and to diminish the cross magnetizing effect of armature reaction.

The value of this central radial opening in the pole piece would seem to be established from the fact that since its introduction, other companies have adopted a similar practice, and with apparently good success.

In regard to the efficiency of the machines herein described, it is clear that there is no resaon why these machines, operating at minimum speed, should be less efficient than any other motor of similar speed and capacity; and with an increase in speed of 125 per cent., these machines carry their full load with an efficiency about 4 per cent. less at maximum than at minimum speed, the reduction in efficiency being due to increased friction and iron losses at the high speed. As the speed control is accomplished without the use of resistance either in the field or armature circuit, the C^2R losses for a given load are practically uneffected by the speed.

Figs. 7 and 8, respectively, show the action of the 6 h.p. motor when operated at minimum and maximum speed, these curves showing the relation of current input to the speed, torque and efficiency.

Comparing first the speed at no load, we find the minimum speed to be 660 and the maximum 1,475, thus covering a range of 123 per cent. variation; and, as screw threaded means are employed for simultaneously adjusting the plungers, it is evident that any imaginable speed between the limits specified may be obtained.

Operated at minimum speed the full load of approximately 50 amps. shows the self-regulation of speed to be within 6 per cent.; while under high speed conditions, the curve indicates that a similar load reduces the speed a little less than 8 per cent. In larger machines the self-regulation is closer than on this 6 h.p. size.

As these motors are designed to develop their full rated h.p.

at any speed within their range, it follows that for a given load the torque must vary inversely as the speed. In other words, in a machine whose maximum speed is twice the minimum, the full load torque at maximum speed will be one-half as great as at the minimum.

Referring to Figs. 7 and 8, we find that operated at maximum speed the efficiency at full load of 6 h.p. is 79 per cent., while at minimum speed the same h.p. is developed at an efficiency of 83.5 per cent., showing a difference in efficiency at full load of only 4½ per cent. This loss of efficiency due to speed variation seems negligible when compared with the loss of efficiency in a variable speed equipment of equal range whose speed is varied by resistance in series with the armature.

Where the range of speed is sufficient, these motors also present the advantage of simplicity over those methods of speed control in which a double wound armature is employed or in which a plurality of auxiliary machines or line voltages is required.

It will be seen that the motor herein described differs from all others in that with constant potential at the terminals, it carries its full rated load at any imaginable speed within the limits of 125 per cent. variation, with an efficiency practically independent of the speed at which it is operated.

It would be a mistake to suppose that this comparatively wide variation of speed is due to the employment of high air-gap density at minimum speed or to low armature reaction by which the weight and cost of the machines would be increased, and as a matter of fact these machines conform very closely to the practice of the leading manufacturers, in the matters of armature reaction and air gap densities.

In these machines the minimum speeds have been chosen much lower than usual for corresponding sizes, so that, with an increase of 100 to 150 per cent. of normal, the peripheral speed shall not greatly exceed that obtained in the moderate or medium speed machines of other manufacturers.

Discussion.

President Steinmetz:—Gentlemen, you have listened to Mr. Packard's paper and discussion is now in order. As you see, the paper describes a method of providing speed control over a very wide range in motors. I understand the author has been largely instrumental in developing this method of controlling speed, and I congratulate him on his success. Discussion is now in order.

Prof. Esty:—The last speaker has referred to two methods previously used to control speed, one due to Mr. Diehl and one to Mr. Edison. I would like to ask if there was not another method employed years ago by the Crocker-Wheeler Company, using hinged pole-pieces, the distance of which from the armature could be varied at will. My impression is that there was such a device, and not hearing it mentioned, I wondered if it had not been overlooked by the author.

Mr. Packard:—I have corresponded widely to find out the names of patentees and also to read the patents which have been issued along this line, and have not succeeded in finding other patents than those of Diehl and Edison, although I am inclined to think that a modified form of machine from that described in the Diehl patent has already been used.

Mr. Thomas:—I should like to ask for information as to experience in the limits of size and speed in this type of machine. For instance, is it possible commercially and mechanically to construct a very low-speed motor of considerable size on this principle; assuming, for instance, the highest speeds, that would be within the range of what is ordinarily understood as low-speed motors.

President Steinmetz:—I am sorry that our friend Mr. Dunn is not here to participate in the discussion of this problem, with which he is specially familiar. I may say that the problem of varying the speed of a continuous-current motor has occupied the minds of electricians for very many years, and has been very carefully studied. There are two ways of varying the speed of a continuous motor, varying the e.m.f. on the armature, or varying the field strength. The first has been developed by our worthy friend, Mr. Leonard. The latter is used to a considerable extent, and is usually accomplished by varying the exciting current. The objection to speed control by variation of the field excitation is the limited range available, since with a decrease of field magneto-motive force the distortion due to armature reaction increases. A somewhat better method appears to be a decrease of field by increase of the magnetic reluctance by means of lengthening the air-gap. While the distortion is not thus increased, the commutating field is decreased and commutation thereby impaired. Mr. Packard's paper shows a further method, by which, without varying the field m.m.f. nor the commutating field, that is, without affecting armature reaction and commutation, the field strength is varied over a wide range.

MR. RAYMOND:—I would like to inquire how large a machine has ever been built on this principle.

MR. PACKARD:—The development of the multipolar form of machine built on these lines is comparatively recent, and only two machines have thus far been built—a 6 h.p. size, the results of which are shown by the diagrams, and an 8 h.p. size.

MR. RAYMOND:—I would like to ascertain the voltage of those two machines.

MR. PACKARD:—As noted on the diagrams, the 6 h.p. was wound for 110 volts; while the 8 h.p. was wound for 220 volts.

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Elected at Executive Committee Meeting, New York, Aug. 22, 1902.

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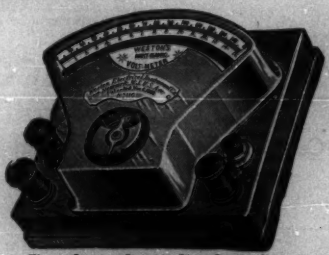

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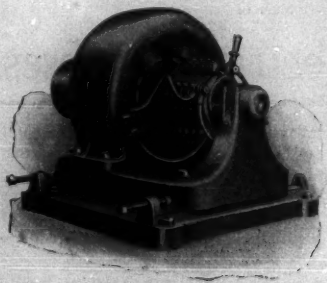

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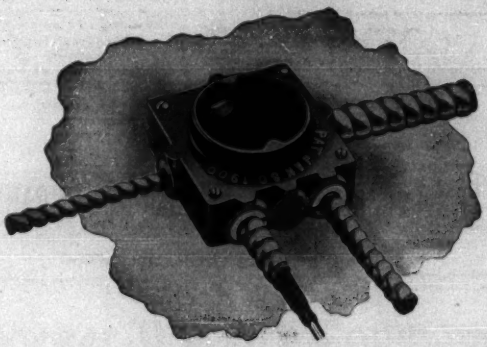

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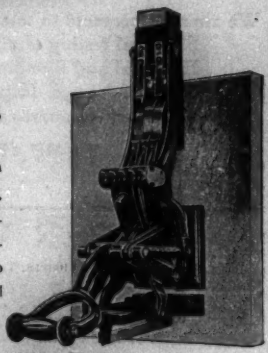

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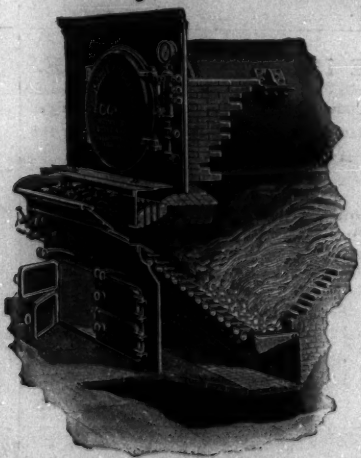

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